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CONTENTS OF No. 29.

	PAGE
Science at the Royal Academy	599
The Three Cold Days of May. By the Editor	600
Crystals. By William Jago, F.C.S., Assoc. Inst. Chem. (*Illustrated*)	601
Photography for Amateurs. By A. Brothers, F.R.A.S. Part VII.	602
Curiosities of Colour. By Henry J. Slack, F.G.S., F.R.M.S.	603
The "Green-Beam" Paper. (A Sequel.) By Lieut.-Col. W. A. Ross, late R.A.	604
Measurement of the Focal Length of Deep Convex Lenses. By T. W. Webb. (*Illustrated*)	605
Butterflies and Moths	606
The Electric Telegraph. By W. Lynd	607
The Comet (*Illustrated*)	608
The Weather Report	609
REVIEWS: "Studies in Microscopical Science"	609
Land and Water	609
CORRESPONDENCE: The "Coney" of Scripture—Probabilities, &c.—610-612	
Answers to Correspondents	612-613
Our Mathematical Column: The Laws of Probability	614
Our Chess Column	614
Our Whist Column	616

SCIENCE AT THE ROYAL ACADEMY.

ANATOMY IN THE SCULPTURE GALLERIES.

A CORRECT knowledge of anatomy is so much more important in sculpture than in painting, though important in both, that in considering this part of our subject we shall do well to turn for a while into the sculpture galleries. As we pass quickly through the other rooms, we note the absurdly long legs in Mr. Pettie's picture of Eugene Aram ("He talked with him of Cain"), No. 18, Room 1; the aged hands of Mr. Wirgman's girl in No. 19 ("I cannot mind my wheel, mother"); the incorrectly drawn right foot in Mr. Cameron's otherwise pretty picture "Evening," No. 48; and the most monstrous foot in Mr. Marks' "A Fugitive Thought." Mr. Pengelley's left hand (Portrait by Cope, No. 79) is manifestly deformed, if the painter has correctly represented it, which we doubt. It would be absurd to criticise the anatomical development of Mr. Melville's "Sower" (No. 98, Room II.), for the simple reason that he has none, nor has he any respect for the law of gravity. In the same room we note, still *en passant*, the singularly unfinished condition of Mr. Starr's portrait of Mr. George Wilkinson, M.A. In Room III., "Bad News," No. 222, a picture of the melodramatic school, by Mr. Marcus Stone, suggests that the trooper who has brought the bad news, and who, if the perspective is correct, must be about ten inches shorter than his mistress, requires a course of athletic exercise to give him a respectable development of chest and shoulders. Ella's hips, in Mr. John Scott's pleasing picture "The Wild Swans" (No. 270), are in danger of dislocation; and in No. 384, Mr. Barwell's "Sweetness and Light," Gallery IV., there are two most hideous, though perhaps correctly-copied, feet, the only excuse for which is that they turn the attention for the moment from the absolutely impossible lilac tint of the riverside scene. Mr. Barrable's "Little Patience," No. 396, in the same room, must be a little opera dancer, if she can keep her feet patiently in the attitude represented. To the artistic eye, the attitudes of ballet dancers are not beautiful; but it is easier to draw feet in the pointed position such folk affect than in one of the many positions which the feet naturally assume. In Gallery V. we note, in passing, that the anatomical development of the late Edwin Christy (No. 433), as presented by Mr. Sidley, would hardly have sufficed for the effective wielding of the sword, some 5 ft. in length, which the artist has bestowed on the hussar. The left arm of the young girl in No. 500, "To be Left till Called For," by Mr. Jerry Barratt, is indescribably incorrect in an anatomical sense; that is to say, it is impossible to tell what the artist meant to give that arm in the way of muscles and tendons—certainly not such as are known to the anatomist—but the crabbed hand may have really belonged to the model. We note in the same gallery a serious fault in an otherwise charming little painting, Mr. Edwin Douglas's "Place of Safety." Such a horse, one with which a dog could safely play, and on whose back a cat could find "a place of safety," never had such an eye. Even without these proofs of a trustworthy nature, the wicked eye would be inconsistent with the rest of the head. Every one acquainted with the ways of horses knows what such an eye means, and how the ears follow suit. In the next gallery (No. VI.) we observe that the bigger dog in Mr. Strutt's "Extremes Meet," is meant to be one of the biggest of the big, but is made, by incorrect drawing, decidedly undersized for his breed.

However, we must hasten to the sculpture galleries if we are to have time to discuss the anatomical defects and excellencies of the works there exhibited.

The first feature which always strikes any one familiar with antique art, in studying the works of modern sculptors, is their unfinished condition. Compare the nude arm or leg of a Greek female statue of the highest type with the nude arm of the best modern statues, and the difference to which we refer will be seen at once. (We mention female statues because there is not in the female figure that obvious muscular development which even the inexperienced can recognise; though in male as in female statues the difference we are considering exists.) By the careful finish of their surface, the older statues indicated not merely the surface contour of the flesh, but the actual form and proportions of the muscles. In modern statues we usually have to be content with a general intimation of the existence of the chief muscles, the delicate gradations of surface which indicate the presence and form of minor muscles being generally neglected altogether. Sometimes even the chief muscles are incorrectly represented. Take for instance Pradier's "Toilet of Atalanta" (sometimes absurdly called the *Venus accroupie*, or Crouching Venus): in this statue, the head of which alone is worthy of Pradier's reputation, the arms have no muscles at all, the muscles of the thigh and calf are not merely incompletely, but incorrectly, rendered, and the flat and long left foot would have been regarded by Phidias or Praxiteles as altogether unfinished,—probably as not worth finishing. Houdon's "Diana" (bronze), compared with a good ancient statue, is like a sketch in crayons compared with a finished painting.

In the sculpture gallery of the Royal Academy we find this year many pleasing works, and many which are suggestive of exquisite beauty, but none (at least of the higher class of statuary) which can be regarded as finished; none, at any rate, bearing evidence of the loving care which the sculptors of Greece bestowed on their best statues. It may be that the fault lies in the want of such models as the Greek sculptors had. Women may be as beautiful in face now as the Greek women of old, but in form they naturally cannot be; for, apart from the use of stays, women have scarce any exercise by which beauty of form can be

XUM

developed, or retained where it already exists. As Pradier's "Atalanta," in her sloping shoulders and pinched waist, bears evidence of Parisian tastes (or of the only type of female beauty which can exist where Parisian fashions prevail), so Mr. Thornycroft's "Artemis," No. 1,644, Mr. Ball's "Lancashire Witch," No. 1,571, and other works in the sculpture gallery at the Royal Academy, bear evidence of the imperfect muscular development of the women of our time. The right arm of "Artemis," for instance, is incorrectly shaped, both upper arm and fore arm, though very likely he correctly represented the arms of his model. This, however, is a fine sculpture, though the idea is somewhat worn. In the "Lancashire Witch," a really charming work, the proportions are more correct, but the limbs are unfinished, or rather the limbs *copied* would have been regarded by an ancient sculptor as imperfect and flaccid. On the other hand, "My Dainty Ariel," by Mr. Armstead, R.A. (No. 1,680), is absolutely and outrageously incorrect in proportions, as well artistically as anatomically. The short fore-arms are out of all proportion to the monstrous hands; the attitude is hideous, bringing out, and, as it were, emphasising, the boniness of the knees; the face is frisky, perhaps—certainly not dainty. Of the wings, as we know nothing of the anatomical arrangements by which wings stuck upon human shoulders could be made to work, we say nothing; but the statue, as a whole, is quite unworthy of Mr. Armstead's repute.

Speaking of winged figures, we are led to notice the representations of "Jacob wrestling with the Angel." Giving priority to the fair sex, we take first No. 1,578, by Emmeline Halse. Jacob is supposed to be at that stage of the encounter where he remarks, "I will not let thee go, except thou bless me." He is certainly not wrestling with the angel. He is holding the angel up from the ground, and his opponent is manifestly conscious that he is in no danger of being thrown, for he is curling up his toes like a child at play. Properly to represent such an encounter, the artist should pass hours in watching *bonâ-fide* encounters between good wrestlers. We will undertake to say that she would never in a real encounter see the toes of a wrestler who has been caught up by a strong opponent, twiddling in the air as her angel's toes are. The actual position taken up by the toes when a wrestler is thus placed (we suppose the opponents, as usual in the North, to be in stockinged feet) is quite characteristic, all the toes closing in towards the ball of the foot; only when the body is actually swayed round for the throw is this position of the toes changed. The toes are not turned upwards when great exertion is made in which the lower limbs take part, any more than the fingers are turned backwards towards the wrist when the muscles of the arm are energetically exerted.

Mr. Robert T. Fallon's treatment of the same subject, No. 1,542, is still less consistent with anatomical facts. The angel (with a face of the type of a Yankee pedlar's, and with an expression disagreeably suggestive of Mr. Julian Hawthorne's impossible pedlar in "Fortune's Fool"), is carefully holding up Jacob with one hand while endeavouring to thrust him down with the other. This is not usual in such encounters. We would remind Mr. Fallon, also, that although certain muscles are called into active exercise in an energetic wrestle, they only become exceptionally developed in persons who give much of their time to such encounters. To give to sculptured figures the peculiar development of men whose chief business is wrestling is to imply that the persons represented were trained wrestlers. There is nothing in the Hebrew record to suggest that either Jacob or the angel belonged to this category. Certainly, Mr. Fallon goes far to correct any erroneous impression in this respect by indicating Jacob's manifest want of skill, and by assigning to the angel the unusual task of holding up his opponent and pushing him down at the same time. But it remains the case that some of the muscles, both of the patriarch's body and of the angel's, have a relative development, such as we only see in veteran wrestlers.

THE THREE COLD DAYS OF MAY.

By the Editor.

IT is a singular, and as yet unexplained, circumstance, that usually in the second week of May two or three cold days occur. And although the fall of temperature is not quite so strongly marked as that which occurs between the 10th and 14th of April—the "borrowing days,"—yet the cold days of May are quite sufficiently marked to be unmistakable. The mean annual curve of temperature derived from half-a-century's observations at Greenwich shows a well-defined depression near the end of the first third of the month of May; and a peculiarity striking enough in its occasional manifestations to attract popular attention, and sustained enough to show through all the variations which have occurred in the weather during half a century, must be regarded as real, not accidental. Its nature is shown in the temperature curve in p. 277 of the second series of my "Light Science," where the peculiarity is far from being exaggerated. Indeed, the curve is softened off on account of the method adopted for tracing it. We see in the temperature for Greenwich a wave-like rise from January to July, the curve sinking then to January again; but the ascending curve is affected by three well-marked depressions, one in February, one in April, and one in May, while, strangely enough, three similar irregularities affect the descending curve in the parts for November and December. It is, indeed, now an established meteorological fact, not for Great Britain only, but for Europe, that during the first fortnight in May the average temperature is considerably below that which might be expected from the increasing elevation of the sun and duration of daylight. It is not altogether true, as I have seen stated in a Continental journal of science, that a week of cold occurs with extreme regularity in the first half of May. It requires but a brief search among meteorological records to find instances of warm first fortnights in May. If we take up any weather summary for a few successive years, we find abundant evidence of the irregularity with which "the cold week of May" makes its appearance. For example, in the summary of the weather given by Gilbert White in his "Natural History of Selborne," we find such records as follows: In 1771 frosty weather to the end of the third week in April, followed by spring weather and rain to the end of the first fortnight in May, and then dry warm weather to the end of June; in 1772 the first fortnight of May was dry, with cold piercing winds; in 1773, throughout May and June, "warm showers"; in 1774 no marked peculiarities; in 1775 warm weather throughout May; and in 1776 cold weather throughout the month; the last half of April, 1778, "snow and ice," followed by rainy weather to June 11; thence warm Mays till 1782, when the first week of the month was cold and dark; in 1783 there was thick ice on May 5; in 1784 cold dry weather during the first twelve days of May; in 1785 mild weather during the first seventeen days, and then cold weather to the end of the month; on May 1 and 2, 1786, "thick ice"; in 1787 fine bright weather to the 22nd, then warm, but on June 7 "ice as thick as a crown piece;" in 1778 a warm dry May; in 1789 a warm moist month; and lastly, whereas May in

1790 and 1791 was a warm month throughout, May in 1792 was cold and bleak. Certainly there is no evidence here of extreme regularity.

I have already pointed out (see last number) that the praises bestowed by the poets of the sixteenth and seventeenth centuries upon the month of May do not relate to the thirty-one days forming the May of our present year, but to those which now fall between May 11 and June 11. The May-day of those times fell, strangely enough, at the very coldest part of what may be called the average cold week of May, but the month of May, as a whole, was then much warmer on the average than our present May, and well deserved Dryden's warm description :—

> For thee, sweet month, the groves green liv'ries wear,
> If not the first the fairest of the year;
> For thee the Graces lead the dancing hours,
> And Nature's ready pencil paints the flow'rs.
> The sprightly May commands our youth to keep
> The vigils of her night, and breaks their sleep;
> Each gentle breast with kindly warmth she moves,
> Inspires new flames, revives extinguished loves.

It is only when the average temperatures of the first fourteen days of May are considered, that we find the now prevalent belief in a "cold week in May" fully justified. The curve of temperature for the year from the observations of the last half century, shows, as already mentioned, a decided depression at the part corresponding to the second week in May, though it is to be noticed that it dips down quite as decidedly at the part corresponding to the second week in April. It appears to me that when we combine the ascertained fact that there is on the average a fall of temperature at this part of May, with the equally certain fact that there is no regularity in the recurrence of the cold week, we must regard as extremely improbable the theory which attributes the peculiarity to a cosmical cause. This theory was thus placidly presented some time ago by M. de Fonvielle as a known truth :—

"The chilliness is due to the fact that the earth passes behind a ring of asteroids, which absorb a portion of the sun's warmth, due to us while it remains above the horizon. The temperature will not resume its ascensional movement until the annual rotation shall have carried our sphere from the shadow of the multitude of the small planets which is always projected on the same point of our orbit."

Next week, I shall point out a few objections to this theory.

POISONOUS CRAYONS.—A little girl, æt. two-and-a-half years, recently died at Brockley from the effects produced in part by sucking poisonous crayons. At the inquest held on the body, the tradesman from whom the things had been purchased disclaimed all knowledge of their injurious properties, and said that he sold them in considerable quantities in penny boxes. Post-mortem examination, however, revealed that the brain and stomach alone of all the organs were in an unhealthy condition. The stomach was much inflamed, and perforations of its coats occurred in two places, while the left side of the brain was distended with fluid. There was evidence that the child had sustained a fall, and to this it was sought in part to attribute the death; but information concerning the accident was incomplete and unsatisfactory, although the jury, by their verdict, credited it in part with the fatal result. Analysis of the crayons conclusively proved that they all contained poisonous material, and there can be little doubt they were chiefly to blame for the death. One of the crayons, a pink one, contained more than fifty per cent. of its weight of white lead, and as the unfortunate little victim lingered for three weeks in much suffering, it ought to be possible to ascertain how far this substance influenced her condition. The newspaper reports give very insufficient details of the case, of which, however, Dr. Kavanagh, the medical attendant, may possibly provide more comprehensive notes. The case is an instructive one, as showing the need for sweeping measures of reform in connection with the indiscriminate sale of poisonous materials of all sorts by general shopkeepers; and in this way it may excite useful discussion.—*Medical Press.*

CRYSTALS.

BY WILLIAM JAGO, F.C.S., ASSOC. INST. CHEM.

No. II.

IN the last paper on this subject directions were given for the preparation of crystals of bismuth and sulphur, in both cases by the solidification of the fused substance. Before leaving the crystallisation of metals, reference should be made to those experiments in which metals are displaced from a solution of their salts by some other element. The well-known "lead tree" is a type of such changes; its formation depends on the fact that zinc is a more active element, chemically, than lead; hence, if zinc be introduced into a solution of a compound of lead with an acid, the lead is "displaced," and its former position usurped by the zinc. The lead is deposited in the metallic state, and under favourable conditions assumes a beautiful crystalline form. The experiment of making a lead tree may be performed with a *minimum* of apparatus and experience of chemical manipulation. An ordinary pickle bottle, or other vessel of clear glass and similar shape being obtained, fill it with a solution of lead acetate (sugar of lead). About an ounce of the acetate will be sufficient for a bottle of the size mentioned; if dissolved in spring water, a slight sediment will be formed—this, however, if allowed to subside, will not interfere with the experiment. Distilled or even rain-water is preferable for making chemical solutions. Next a fragment of *clean* zinc, about the size of a small walnut, must be procured, the more irregular the better; suspend this by a piece of string, the end of which passes through a hole bored in the cork of the bottle containing the acetate. Put the zinc in the bottle, cork it up, and so arrange the length of the string that the zinc is just beneath the neck; fasten it in this position, and set the whole arrangement where it will not be disturbed. In a short time crystals of lead will be seen to deposit themselves on the zinc, and soon it will be covered with a tree-like growth of crystals. If left perfectly still, it remains a long time before the mass drops off. With this and other similar experiments, one half the pleasure consists in watching and studying the crystal growth for one's self.* Silver, which is in many respects a metal closely allied to lead, may also be made the subject of interesting experiments on crystallisation; the so-called *Arbor Dianæ* is produced by placing a globule of mercury in a solution of nitrate of silver; a growth ensues of long thin crystals of an amalgam of silver; these, in addition to their beauty of shape, possess that magnificent lustre which causes mercury and silver to be almost unrivalled among the metals.

Those who possess a microscope will find a few prepared specimens of crystals a valuable addition to their stock of slides. Not only are they of great interest, but as an introduction to microscopic analysis and microscopic study of rocks, the systematic student will find them worthy of special study. It has been previously stated that the crystalline form of many substances is one of their most characteristic properties; and as in the detection of poison and other important cases there is often a trace merely of the substance to be obtained, a microscopic examination is of great importance; it has, too, this further merit, that the substance is afterwards available for other chemical tests.

The preparation of such slides is very simple. In the first place the glass slips must be perfectly clean and free from grease; it is well to wash them in a solution of soda,

* It should be stated that lead acetate is a poisonous salt. In all cases chemicals should be kept clearly labelled and locked up.

XUM

rinse with rain or distilled water, and then wipe dry with a clean linen cloth. The substances that may be selected for study are legion; those figured are very suitable for a first attempt; make solutions by putting a pinch of common salt, potassium nitrate (saltpetre), oxalic acid and potassium dichromate in separate clean test-tubes, and add to each a tea-spoonful of water, they will dissolve rapidly to clear solutions. Take a drop out of the common salt test-tube on the end of a glass rod, and place it on a clean slide, spread the drop out with the rod in as thin a layer as possible; warm the slide very gently over a lamp until the salt begins to crystallize round the edge of the drop, then place it under the microscope and watch the progress of crystallisation. Little cubes of salt will be seen to form, and ultimately the field will appear as shown in Fig. 1.

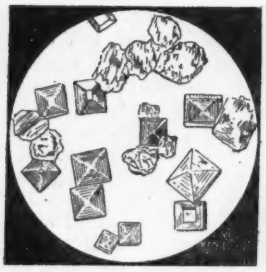


Fig. 1.—Common Salt.

Fig. 2.—Potassium Nitrate.

Precisely the same experiments being made with the other solutions, the shapes of the respective crystals are shown in the accompanying figures. Potassium nitrate differs remarkably from the salt; instead of the little cubes, we have the crystals arranged in long parallel feathers. The oxalic acid, again, shows forms differing from the other two; from a centre the crystals radiate out in every direction. Of the four specimens, however, the potassium dichromate is the most beautiful; the crystals, instead of being colourless, are of a deep amber hue, while, in mode of arrangement, they resemble a fern group rather than mere inanimate matter. The figures must be looked on as

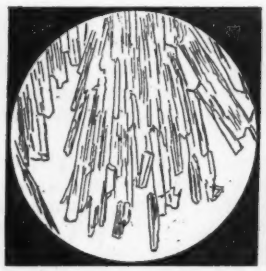


Fig. 3.—Oxalic Acid.

Fig. 4.—Potassium Dichromate.

giving some idea only of what is actually seen. The leading outlines have been drawn, but to copy the delicate tracery of the finer crystals is impossible. In the case of the potassium dichromate in particular, some parts of the field defy all attempts at even affording a conception of their exquisite beauty. But were even all this possible, there is yet the greater charm remaining to the actual worker, and that is to see the growth proceeding. The specimen being so placed that the edge of the crystals already formed is just within the field, the main lines shown are first rapidly filled in, and then the smaller branches dart out until, the water having evaporated, the whole of the salt has regained the solid state.

PHOTOGRAPHY FOR AMATEURS.

By A. Brothers, F.R.A.S.

PART VII.

THERE are some advantages in trimming the prints before toning. One is, that the black edge would take up gold wastefully, and as the paper contains silver, the waste edges should not be destroyed. The saving, where the quantity of paper used is large, amounts to an important item in the course of a year. A third advantage is, that the prints may be mounted at once. Prints on albumenised paper have a habit of curling, and it is, therefore, less troublesome to mount them while damp. The medium for mounting may be gelatine or starch. The starch may be of about the same consistence as is used for stiffening linen, and should always be used fresh. The only disadvantage in the use of starch is, that if it be required at any time to remove the print from the mount, there is some risk of spoiling the print; but it may be done by steeping in hot water, and gradually stripping or rubbing the paper or cardboard from the back of the print. Prints mounted with gelatine are readily detached from the mount when placed in hot water. The mountant should never be used if at all acid.

When dry, the prints should be pressed with a hot flat-iron, a piece of smooth paper being used between the print and iron. But a better surface will be obtained by passing the prints through a suitable rolling-press. A solution of indiarubber is sometimes used for mounting prints, and it has the advantage of not cockling the paper, but the prints are liable to peel off after a time. There is always a difficulty in mounting prints on paper, or even cardboard, unless very thick, on account of the cockling caused by the contraction of the print in drying. If the prints are larger than 8 in. by 6 in., they have a much more finished appearance if mounted on plate-paper with an india tint; but this cannot be done by an amateur, and the prints should be sent to a professional mounter, the cost being very little more than if done at home on cardboard, and the superior finish is well worth the extra cost.

Up to this point I have endeavoured to describe a process by which photographs may be obtained, and if the directions are carefully followed, a very little experience will enable the student to become expert. The chief difference between the amateur and the professional photographer is that the latter has more practice, but there is no reason why the work of the amateur should not equal that of the professional. It may happen that our pupil has no friend to whom he can apply to help him over difficulties. By a recent change introduced into the management of the correspondence columns of Knowledge, we offer to take the place of the friend, and we hope during the present season to welcome many workers in one of the most fascinating of arts.

Before closing this part of our subject, a few general remarks may be useful.

In the early days of paper prints, the skies of landscapes were almost always left white, but the effect was most inartistic. It is now rare to see a landscape without appropriate clouds, often taken at the same time as the rest of the picture. This, however, is not always possible, and for one reason, amongst others, that the sky may be quite clear when our view was taken. It is quite easy to make

photographs of clouds, and no opportunity should be lost to obtain negatives of various size, or they may be taken on small plates, and afterwards enlarged. These negatives are to be used for *printing in* the skies. Always be careful to select a cloud negative with the light falling in the same direction as in the picture. Place the print on a flat board or sheet of glass, and then put over it the cloud negative, carefully arranging it so that the masses of cloud fall suitably. Cover the lower part of the print with a piece of cardboard, so that no light can injure it; but the top part of the cardboard must be bent upwards, permitting the light to graduate towards the horizon of the print. Now expose to diffuse daylight to print the clouds. This will very soon be done, as the printing must not be carried too far, or the results will be heavy and unpleasing.

Success will depend very much on care and cleanliness in all the various operations described. The dish used for nitrate of silver solution should not be used for any other purpose. All bottles should be carefully labelled, and always put in proper places. Glass measures should always be washed out after use. The bottles containing collodion, developing, fixing, and other solutions, should always be kept in the same places while in use, and preferably should be of different shapes, so as to avoid mistakes in the feeble light of the darkened room. The glass vessel, or bath, containing nitrate of silver solution, should have a wooden case made to slope at a suitable angle. This case is to protect the collodionised plate from light caused by the chance opening of a door, and if the top be protected by a suitable cover, the plate may be safely left with a flood of white light in the room while the operator proceeds with other work. The dark slide of the camera should be wiped dry after using each plate. Thick blotting-paper may be used for this purpose. Nitrate of silver stains on linen are troublesome to remove, therefore keep certain cloths for certain purposes. To remove accidental stains, a strong solution of cyanide of potassium should be used, and the parts should then be washed with soap and water. Stains on the hands may be removed by rubbing them with solution of iodine and then with cyanide of potassium. Dilute hydrochloric acid will also remove stains from the skin.

CURIOSITIES OF COLOUR.

By HENRY J. SLACK, F.G.S., F.R.M.S.

INTENDING to resume the "Studies of Minute Life" by a notice of the micro-ferments and M. Pasteur's discoveries, the writer may be permitted to indulge in a temporary change of subject, and mention some interesting experiments with colour, in the hope of thus supplying answers to some of the questions that have been put by readers of KNOWLEDGE.

A couple of prisms will enable two spectra of the sun to be thrown upon a white ceiling, or sheet of white paper fastened to the wall. By a little management, the two spectra may be wholly or partially superposed, and the very different behaviour of light rays from pigments thus displayed. The exact and coincident superposition of two similar spectra will reinforce the colours, and if one is thus made to slide over the other, the effects of combination will by no means agree with the mixtures of analogous pigments. The red and the green will give a yellow, and this result of combining red and green is also made evident by Lord Rayleigh's "New Colour Combination Plates," made by Browning. In this apparatus a blue disc is superposed over a yellow one, and then, as the inventor says, "the yellow disc absorbs the blue light. The blue disc absorbs the yellow and orange light. By superposing the discs, and viewing a white object, as a cloud, the resultant light will be yellow. But on analysing the same with a spectroscope, it is proved that yellow light is produced by a commingling of red and green, since these rays of the spectrum are alone transmitted."

The success of the experiment depends upon hitting the exact tints of the two colours. In the specimen before the writer, the yellow is rather orangey, and when examined with the spectroscope shows colours from deep-red to a blue-green. The blue disc analysed in the same way shows a band of bright red, succeeded by a cloudy one, something like a dingy, brownish puce, and then green, blue, and violet; no yellow. The combination, which gives an orangey yellow, when analysed shows red and green, and a cloudy band in place of the orange and yellow. The blue and violet are suppressed.

Another way of making interesting experiments is by obtaining from an artists' colour-shop (such as Brodie & Middleton, in Long-acre) various sheets of coloured gelatine and a sheet of black cardboard. Cut some of the cardboard into 4-inch squares; make a round hole in the middle of each square, 1½ inches in diameter. Over these holes fasten coloured gelatine of all the chief obtainable tints. This is done most usually by laying the gelatine on the unblackened side of the cardboard, and gumming over it a square paper with a hole in it corresponding with that in the cardboard. This keeps the galatine in its place. When one of the squares so prepared is held near the eye, the black part excludes most of the diffused white light, and objects in a garden or in a room may be seen as affected by the tint of the gelatine, with a result that is often very different from what might be expected. For example, a sky-blue disc slightly blues white flowers, gives a dingy purplish hue to the rich pink of dielytra, and makes the yellow-green of aucuba leaves less noticeable. After this take a disc of purple, like the common small blue glass so often misapplied in cockney conservatories. Many green leaves now flash out with red tints. At this moment the writer sees this effect in young April rose leaves, the lightest aucuba leaves, the golden green of *Thuja aurea*, and many more. Polyanthus and winter aconite leaves assume a dull lurid tint, while the dielytra blossoms have a richer glow. A crimson disc quite changes the bright leaves of aucuba and *Thuja aurea*. This shrub looks as if covered with orange-yellow flowers, while the sky, now covered with a pale cloud, takes the full crimson hue.

All the objects at the time of writing are in a fairly strong white daylight, and the leaves, according to their character and the angle under which they are seen, reflect more or less white light, besides exerting their normal effects of absorbing portions of the spectrum.

For another set of experiments, cut some pieces of black cardboard six inches long and four wide; punch, or cut, then, half-inch in diameter round holes. Cover one set severally with a blue, a crimson red, and a yellow piece of gelatine. Hold it up against a white cloud; look at it with a double image prism. By keeping the prism near the eye, and holding the cardboard at a suitable distance, to be found by experiment, and then rotating the prism, it is easy to make any one of the coloured discs overlap any other one. Sky-blue and pale yellow thus treated give nearly white light. If the same tints of pigments were mingled, the result would be a pale green. The yellow superposed upon the crimson makes it orange. Superposing the pale blue upon the crimson gives a whitish, violetish grey, the tint varying with the quantity of white light reflected from the cloud.

XUM

Prepare another card by occupying two holes with discs of blue and yellow, each having one, and the third hole with both colours superposed. The result of this superposition is a green, as it would be with pigments, and it is interesting to compare this green with the approximate white obtained by superposing the tints by means of the double image prism. A similar card prepared with crimson and green separate, and also the two combined, are likewise instructive. The actual superposed combination of the gelatines effects an orangey modification of the crimson, but when the superposition is made by increase of the prism, yellow is the result. Other combinations by superposition and by prism should also be compared.

After the above experiments, gum three strips of white paper, about one inch long by half-an-inch wide, on a square of black cardboard, a little way apart. On one slip fasten (which can be done by slightly moistening) a square of blue gelatine and one of yellow beside it. Expose to full white light, and cause superposition by the prism. The tint obtained is a chocolate grey. Red and crimson thus tinted give orange; crimson and green, dark grey.

The effect of a double image prism, by dividing one coloured ray into two, is to lessen its force, and the effect of adding, by reflection or transmission, a strong white light to any colour is to thin it out, or completely subdue it. Stick a small square of crimson gelatine in the middle of a sky-blue disc fixed in a black cardboard square; hold it against a white cloud; duplicate the image of the crimson square with the prism; such image is bluish-grey. Make portions of the two images touch; the crimson, modified by the blue, at once appears.

In studying these various effects, the exact conditions of each experiment must be noticed. Thus, dealing with spectrum colours is employing rays of light of certain refrangibilities, according to the dispersion obtained. Pigments are either opaque or transparent. The action of an opaque pigment is to absorb certain rays composing white light, and to reflect the residue. If such a pigment is seen in strong white light, the quantity thereof which it reflects acts in proportionate diminution of its normal colour. In viewing gelatine pastes held up against a white cloud, the colour seen is the residue left after its absorption of the rays that cannot pass through it. If one film is imposed over another, and both are held up to the light, the first film only transmits to the second the rays that remain after its absorptive powers have been exercised. If a transparent tint is superposed upon white paper, that paper reflects through it the rays which the coloured film has not absorbed, and subjects them to a fresh action of the film as they pass for the second time through it.

To show what white, or approximately white light can do with a strong colour, throw a full red on a white screen with a magic-lantern. Then with another magic-lantern, or a common bull's-eye lantern, placed nearer, and thus casting an intenser light, overpower the coloured light. A hand with outspread fingers, held so as to obstruct the white light, casts a red shadow. The stoppage of the white light permits the redness to reappear. This might be used in melodrama with powerful stage effect. A black figure could be made to cast a blood-red shadow as he passed a white-looking wall. The experiment in a room with the outspread fingers is very striking. Many similar arrangements will occur to your readers, and among them not the least interesting is the production of subjective tints by throwing with a lantern on a screen the images obtained by Wheatstone's superposed discs of perforated zinc. It is easy to fit in the ordinary wooden slide-holder a square of the zinc, which may be fastened to the frame, and to fix to it a circle of similar zinc, so as to rotate freely on a pin. The circle should be wide enough to project a little above the frame, and be easily reached with a finger, to make it revolve. Coloured gelatine films placed close to the front lens of the lantern will give the effects required.

"THE GREEN-BEAM PAPER."

(A Sequel.)

By Lieut.-Colonel W. A. Ross, late R.A.

A SUNNY day—"happy Saturday," April 8—which has intervened since the publication in Knowledge (page 496), of the article, aptly named by you as above, having enabled me to add a few more auxiliary experiments to the original one there detailed, made on the 7th of October last, I would ask of your courtesy a little space in an early number of our delightful "Mag." for a brief description of them before your readers forget my former paper.

1. A plate of blue-violet glass, 0·162 inch thick (the thickest I had), transmitting blue-violet light, but from which light, so transmitted, a sheet of white paper reflected reddish-violet light, was held in the path of a sunbeam admitted through the open window of a scarcely-darkened room, and condensed by a lens about 2 inches in diameter, in two positions—(*a*) so that the sunbeam was focussed *on* the glass; (*b*) so that the sunbeam was focussed on the paper about half a foot beneath it, *through* the glass. In the case of (*a*) the solar image on the paper was large of course, and oval, but of a pure blue colour; being apparently the result of the elimination of red rays by the thick glass, and of yellow rays by the blue part of the blue-violet glass, leaving the blue rays only for transmission. In the case of (*b*) the focussed solar image was small, nearly circular, and of a pure blue colour, contrasting strongly with the violet transmitted to the paper by the rest of the glass. As the mixture of red and blue lights, in certain proportion, undoubtedly forms violet light; and, as blue or bluish glass intercepts yellow rays, this blue colour was, on this hypothesis, to be expected, and it was supposed by me that the purity of the blue focus on the paper was due to the thick glass cutting off red rays, according to the experiment detailed in Knowledge of April 7, and to the blue in the glass cutting off yellow rays, leaving only pure blue rays to pass on to the paper.

The focus (*a*) appeared at first of a pale orange colour on the surface of the glass, but soon became greenish, perhaps from the simultaneous transmission of some reflected blue rays given out by the main body of the glass to the retina.

2. A similarly tinted violet glass, but only half the thickness of 1, was held, with reference to the lens and paper, in the positions 1 (*a*) and 1 (*b*); when – (*a*) there was a large violet image on the paper, but with a distinct reddish border.

(*b*) The focussed solar image was small, and of a faint bluish-violet colour, so extremely pale as to be nearly white, and tolerably brilliant as reflected directly from the paper; but this bluish-white brilliant image, viewed through the glass itself, appeared of a pale violet colour, with a bright pure red or "crimson" border. *No red* was observable through the glass of 1 (*b*).

3. After dark, a thick platinum wire, bent at a right angle, and heated to faint redness before a blowpipe, was viewed through glass 2, when the "blue" blowpipe pyrocone appeared a red violet, and the wire a brilliant red colour, similar to the border 2 (*b*), which became bluish white when the wire was made white hot.

Am I wrong, then, in supposing that the thin glass 2 did not effectually cut off the red rays, some of which, therefore, escaped through it from the lens to the nearly white focus on the paper, and were contained in the reflection of that which passed directly to the retina, although invisible there; but when the violet glass intervened, the red rays were seen to be partially separated from the blue rays in their vibrations through the glass, and placed as the outer border of the pale-violet focus?

4. A green-blue glass, the thickness of 2, was treated with sunbeam and lens similarly to 1 and 2, but the only remarkable phenomenon I could observe was that the rays reflected from the focus on the glass in position *a* were an almost brilliant reddish orange colour.

5. A slip of thin ordinary glass (a microscope slide) transmitted the focussed sunbeam as white light through its shorter axis, but when turned edgeways, the "green beam" immediately appeared, and it could even thus, by turning the glass slowly round, be ascertained to what depth the beam could travel as white light; so that it would appear as if the red rays were absorbed, and not "cut off"

by the vitreous medium; and glass, I believe, is known to absorb the ultra-red rays.

6. A thin dinner-table water caraffe, full of pure water, transmits the focussed sunbeam as white light.

7. (*Easter Sunday*).—This fine sunny morning I really believe I have proved my point by eliminating, or nearly eliminating, the red rays from the solar spectrum thrown on a sheet of white paper, by carefully introducing "the green beam" into the prism; when, instead of the broad red band, an oval spectrum appeared, full of pale bluish light, with a broadish yellow, and an extremely narrow, faint, red border, the latter attributable, I think, to the imperfection of my apparatus, for my little room was full of white light and even sunbeams, which were also copiously reflected to the prism from the sheet of white paper on the floor.

After all this, some of your readers may ask "what *is* 'your point'?" I will, therefore, conclude by answering (1) that I believe glass of ordinary density, and of considerable thickness, absorbs the least refrangible (or red) rays of white light which has already been refracted through a lens; and (2) that Sir David Brewster was not far wrong when he asserted that white light is composed of three primary colours, red, blue, and yellow; from which all other colours can be produced.

(*May* 7, 1882).—Since the above was written, last Easter Sunday, having been to Germany, *viâ* Holland, to place my two little daughters at school in Coblentz, I took the opportunity of showing, or communicating the "Green-Beam Phenomenon" to Professors H. A. Lorentz, of Leyden University; M. Kekerlé, of Bonn University; and Bruno Kerl, of the Imperial Mining Academy, Berlin, who has published articles on my new system of Blowpipe Analysis in the *Berg und Hüttenmannische Zeitung*.

Dr. H. A. Lorentz, to whom I showed the phenomenon at Amsterdam on Saturday, April 29, was astonished that such a startling phenomenon as the "Green-Beam" had not been discovered before, but expressed still greater surprise when I told him I had been trying in vain to get an account of it published in the proceedings of our scientific societies for six months!

I have now made the following additional experiments:—

8. I have passed the Green-Beam through a thin glass flask filled with sulphate of quinine solution, and *vice versâ* (to see if it was due to fluorescence).

9. I passed it into a cobalt-blue glass paper-weight at least two inches thick.

10. Into a thin glass flask containing a blue solution of ammonia-sulphate of copper, and *vice versâ*.

11. Reflected the focused beam from a small mirror upwards into a thick glass paper-weight.

12. Passed the Green-Beam through one solid glass paper-weight into a prism.

I must, however, reserve the results of these experiments for another paper.

After the above article was in type, Prof. G. G. Stokes was so kind as to examine the "Green Beam" at the Royal Society's rooms, on May 11 (although there were only glimpses of sunlight), and to refer me to Sir David Brewster's papers "On the Decomposition and Dispersion of Light," in the *Philosophical Magazine*, &c., which, he says, must be determined to contain the first discovery of this phenomenon, termed "Internal Dispersion," by Brewster. Here are Sir David's own words (*Phil. Mag.*, 1848, Vol. XXXII., page 403):—"In order to observe the phenomena of dispersion most distinctly, I transmit a condensed beam of the sun's light through the specimen when partially covered with black wax or velvet. . . . page 404. I have found several glasses which possess internal dispersion—one in particular, of a yellow colour, which disperses a brilliant green light. . . . In these cases the glass has a decided colour of its own; but I have found many specimens, both of colourless plate and colourless flint glass, which disperse a beautiful green light."

Some of these words most decidedly describe in part the phenomenon I thought I had first discovered. But there is also a great difference. Sir David evidently only experimented on (comparatively) thin plates of glass (those were not the days of massive glass paper-weights); whereas the "Green Beam" can only be properly exhibited in blocks or cubes of solid glass some 2½ inches thick. Secondly, it is evident from his drawings and descriptions that he merely passed the sunbeam through the thin glass, whereas I focussed it upon the surface of the thick. In the first case there is no green beam at all, but only a kind of diffused green light, much less brilliant than the beam.

That the phenomenon of the green beam is not the result of dispersion but of transmission, seems to me provable by the facility with which it burns black cloth when passed through a crystal paper-weight 2½ inches thick. Who ever heard of burning cloth with the green colour dispersed from a solar beam by a prism? Finally, I think it is also provable that the green beam is due to the union of blue and yellow rays, by the fact that if the thick cubic crystal paper-weight is covered by a plate of yellow or orange glass—which cuts off the blue rays—you have no longer a green beam by focussing on the surface, but only a faint yellow one; whereas the green beam is invariably transmitted through violet, pink, and other coloured glasses, which are not opaque to blue rays, with the exception of red glass,* through which no beam at all is transmitted into the paperweight; *i.e.*, white light seems to be re-formed.

MEASUREMENT OF THE FOCAL LENGTH OF DEEP CONVEX LENSES.

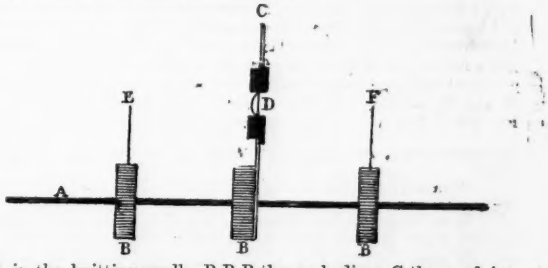
By T. W. Webb.

THE difficulty of ascertaining the focal length of small convex lenses within reasonable limits of error is so considerable, and the result is often so untrustworthy, that I am induced to ask the editor's sanction for the appearance of the following extract from one of my old note-books. Its value, I fear, is not in proportion to its length, on account of the needless minuteness of its detail; all that is worth preserving might have been compressed within much narrower limits; but, possibly, those who, like myself, are fond of manufacturing optical contrivances with ordinary materials may find in their own experience some apology for its tediousness.

T. W. Webb.

Account of a Method of finding the Focal Length of a very small Lens, employed by me, November 24, 1835.

As this process may be useful, on many occasions, both to myself and others, I intend to give an account of all the steps of it. The deepest eye-piece of my 5½-feet achromatic is marked by Mr. Tulley 250. To increase the power I had frequently unscrewed the field-glass, and used that next the eye only; and my object was to find the power thus obtained by measuring the focal length of the lens. There were, however, considerable obstacles in the way. It is very difficult to measure the focus by receiving the image of the sun on paper, because the spherical aberration of a deep lens is so considerable that it is hard to say where the image is best defined; and this uncertainty is greater than might be imagined by a person who has never made the trial. I attempted to measure the image of the object-glass formed by this eye-lens with a micrometer, upon the principle of the dynamometer; but this image was formed so close to the eye-lens that the hairs of the micrometer could not be brought near it. It then occurred to me that if I could ascertain the distance between the conjugate foci when they were equidistant from the lens, one fourth of that distance (by Prop. XLV. of "Wood's Optics") would be the (principal) focal length. To attain this object by the simplest means, I took a knitting-needle, and having bored holes through three thick discs of cork, made them to slide upon it. To the centre disc was glued a piece of card, into a hole in which I stuck the brass cell containing the lens. In each of the other discs I stuck a piece of a broken sewing-needle, so that its end might range at about the same height with the centre of the lens. This apparatus is represented in the following sketch:—



A is the knitting-needle, B B B the cork discs, C the card into which was stuck the brass cell carrying the lens D, E and F the pieces of sewing-needles. Then, by the above-mentioned proposition, if E and F could be so arranged with respect to D, that, while they were equidistant from it, the image of E should be formed at F, and of F at E, then the distance from E to F would be four times the focal length for parallel rays. I therefore took a deep lens in my hand, and while I viewed F in its focus, moved E to different distances, until its inverted image was seen in conjunction with F.

If then E and F appeared to be equidistant from D, they were in

* The red glass I have is red only on the upper and under surfaces; internally it is colourless. All my other coloured glasses are homogeneous.

XUM

the required position, or sufficiently near it; if not, both would require to be moved a little so as to satisfy the above condition. When that was obtained, all that remained was to take the distance between E and F by a pair of compasses, and determine its value from a scale. I have an excellent ivory scale, divided into fortieths of an inch; and to save trouble, I assumed this quantity as the unit of my measurements. Having made the simple and rude arrangement described above, I attempted the operation, E and F being stuck with their points into the cork discs. My first trial, however, proved that under a deep lens their blunt ends appeared too coarse to admit of accuracy. I therefore stuck them the other way into the corks, with the points upwards, which answered much better. My first trial, with the blunt ends, gave the distance from E to F 30 parts of the scale; the second, with points, 31; both by candlelight. The next morning five trials gave 31·166, 31·666, 31·0, 30·666, 31·0. (When the distance taken by the compasses did not exactly correspond to any division of the scale, I repeated it on a paper by the edge of the scale till a coincidence was obtained, upon the principle of the vernier.) The lens was plano-convex, and these trials had been made with the convex side towards the eye; it struck me that, on account of the aberration, it ought to be turned the other way, which was done, though, in fact, it was needless, as I was mistakenly reasoning from the case of parallel rays. I then commenced a fresh set of trials, which gave me the following results:—32·0, 31·333, 31·5, 31·5, 31·25, 31·8, 31·5, 31·3, 31·6, 31·6, 31·6. It should be mentioned that, after every trial, the discs carrying the needles were moved from their position, and the succeeding trial was commenced entirely afresh. It was soon apparent that the method deserved confidence, and the results agreed more nearly as my eye and hand became accustomed to the operation, the last three exactly coinciding; whence I preferred their value, as an approximation to the truth, to a mean of the whole, and deduced $\frac{31{\cdot}6}{4}=7{\cdot}9$ (fortieths of an inch) as the focal length of the lens for parallel rays.

I will now mention the difficulties and the advantages of this method. The greatest difficulty is in measuring the distance between the points of E and F with compasses. This, perhaps, could only be done by a very near-sighted person like myself (or by the aid of suitable spectacles), and required practice and steadiness of hand. Another source of error lay in the possibility that the points E and F might not lie in the axis of the lens, but might be originally fixed too high or too low, or might slip on one side, as, indeed, frequently happened from the turning of the cork discs upon the knitting-needle as an axis. Another difficulty consisted in estimating the position where E and F should be equidistant from D. The situation of the latter in (the depth of) its cell could not be very plainly seen, and as its thickness was considerable, it was uncertain from what point in it they should be equidistant.

It must be observed, however, that every one of these sources of error is to be ascribed, not to the method, but solely to the defects of so rude and imperfect an apparatus, which might easily and completely be remedied. A microscopic eye and steady hand were the only means I employed to obviate them, and, as the results show, not without success. As I became more familiar with the operation, I guarded more carefully against the second source of error; and hence, probably, arose the increasing coincidence of the later determinations; the third difficulty, which I found least remediable, was not likely to produce any injurious effects. For calling x the principal focus between E and D, and y the principal focus between D and F, we have, by Cor. 3 to the before-cited Proposition,—$Ex : xD :: Dy : yF$. Supposing now that E has been placed at a distance of 14·8, instead of 15·8, from D, we have $yF=\frac{xD\times Dy}{Ex}=\frac{7{\cdot}9^2}{6{\cdot}9}=9$ nearly, and EF will be $=(6{\cdot}9+7{\cdot}9+7{\cdot}9+9=)\ 31{\cdot}7$, instead of 31·6, an inappreciable difference. Or if ED was only 14 (and a greater error is very unlikely, since that would make DF = 18·1), still EF would be only 32·1, which would make but about 0·012 of an inch difference in the focal length.

The advantages of the method are—1. The accuracy with which the place of the conjugate focus may be determined. If the image of E is brought so that its point may be directly over, and close to, that of F, an eye accustomed to telescopic observation will determine, without much trouble, the situation in which both are most distinct at the same time; and this might be reduced to greater certainty, if the lens with which they are examined is fixed in a slide instead of being held in the hand. 2. The elimination of the thickness of the lens whose focus is to be measured—a very troublesome quantity, which it is a great advantage to get rid of. 3. That the errors of observation are diminished to one-fourth in the final result; the measured distance being four times the focal length required.

BUTTERFLIES AND MOTHS.

THE science of Entomology, during the last few years, has made enormous progress, both in regard to the number of its devotees, and also in the discoveries of the best methods of obtaining and preserving the insects. We purpose, therefore, giving a few notes weekly relating principally to the order Lepidoptera, about the haunts, times for catching, food, &c., of our British butterflies and moths, which one may generally expect to find during the course of the year.

The present may be called the opening month of the season, and the entomologist will find plenty of work to do, both day and night, in the lanes and woods of the country. A bright May-day will most certainly bring numbers of butterflies to the net of the energetic collector who keeps his eyes open, and who can manage a sharp walk over fields and lanes without feeling the fatigue.

The first thing for the would-be entomologist to do is to obtain a net, a few setting-boards, some entomological pins, and two or three dozen chip-boxes, and then he can consider himself well set up to begin with. All these articles he can make himself, with the exception of the pins and chip-boxes, which can be obtained at a small cost of the dealers in natural history implements and specimens. The setting-boards mentioned are pieces of soft pine about fourteen inches long, and of various widths, with a cork face, and a groove cut down the centre.* Entomological pins are a very fine and sharp-pointed kind, made specially for this purpose; they can be obtained in assorted sizes at 1s. per ounce, one ounce lasting most people an entire season.

We will now suppose the insects to have been caught, and the momentous question of how to kill them crops up. For butterflies, the readiest method is to use a killing bottle composed of cyanide of potassium, covered over with a layer of plaster-of-paris in a wide-necked bottle; the insect is killed very quickly, but unfortunately the wings get set equally rapidly. Another way is to squeeze the thorax of the victim whilst in the net. This kills immediately, but has the disadvantage of slightly damaging the insect. For moths, the best plan is to stupefy with chloroform, and then prick them just below the *thorax*, or the under side, with a sharp pen dipped in a saturated solution of oxalic acid.

Setting is the next process. It is accomplished in the following manner: One of the entomological pins is thrust through the moth near the head and the wings stretched out,* and then a narrow strip of cardboard pinned over to keep them in position. This operation requires great care and neatness, and must be done very shortly after the death of the insect, or the wings get *set*, and cannot be moved without considerable trouble and delay, and very often damage to the moths during the process of relaxation. This latter will be described in a future paper.

The insects must be left on the setting-boards for a period varying from four or five days upwards, according to the size of the moth or butterfly and the length of time the boards can be spared. Personally, whatever the size of the moth, we let it remain until we require the board again, which is generally at least a month or six weeks after, as we keep a large number f boards in use.

The different stages of the work required to be done in killing and setting the insects having been described, we will pass on to the manner of keeping the specimens in good order and condition.

Our first advice to the tyro is, not to get a cabinet unless he can afford to buy a thoroughly good one, with air-tight and dust-tight drawers.

By far the best plan is to use store boxes of moderate size, either plain or covered with green baize, as they are perfectly air-tight, and do not allow the light to penetrate, which is a very great consideration, owing to the colours of some moths being very unstable, greens and other light colours often becoming white in a very short time, owing to exposure to the action of light. However, if getting a cabinet is decided on, we repeat, *let it be a good one, with well-fitting drawers.*

The arrangement of specimens depends to a great extent on the taste and fancy of the collector; but the most scientific and satisfactory method is to follow the order of *genera*, *families*, and *sub-families*, which the best naturalists have adopted. Next week we will give this classification in full, as space forbids in this paper. A piece of camphor about the size of a walnut must be put in each box or drawer to keep off the mites, whose ravages have sometimes, in one single winter, destroyed or damaged the whole work of the preceding summer, when this precaution has been neglected.

Everything now is done connected with the indoor work of the entomologist, and next week we purpose describing the different processes used for catching both diurnal and nocturnal Lepidoptera, together with their habits and other useful information.

* Diagrams illustrating the above will be given in our next number.

THE ELECTRIC TELEGRAPH.

By W. LYND.

CIRCUITS AND GALVANOMETERS.

IN a previous article I explained that when the wires attached to the metallic plates of a simple voltaic cell are joined together, chemical action begins, and a current of electricity is said to flow from the zinc through the liquid to the copper, through the wire, and back to the zinc plate. Without a complete circuit, telegraphy is impossible. For instance, suppose that a wire is connected to the terminals of a battery, and carried on poles to its destination—say from London to Birmingham—and the extremity of the wire attached to the terminals of an instrument at the latter place, no current would be received at Birmingham. If we lay on a water-pipe from a reservoir to a private house, water will flow through the pipe, and it can be drawn off by means of a tap, when required. Compare the voltaic battery with the reservoir, the conducting wire with the pipe, and the difference will be clearly understood. From the reservoir, one pipe is sufficient to enable the water to flow, but the voltaic battery must have a wire or conductor in connection with its positive pole carried to its destination through the instrument and back again to the negative pole; the current must make a complete circuit, no matter how great the distance may be between the two poles of the battery. In actual practice, the return wire is dispensed with, and the earth performs the functions of a second wire, and completes the circuit in a very remarkable manner. To make what is called the earth circuit perfectly clear, take a voltaic cell, and instead of bringing the wires in contact, connect them with two plates of metal buried in the earth, or, for convenience sake, join the wires to the gas or water-pipes; the current will flow as before, and in exactly the same direction. The earth actually plays the part of a metallic wire, and the electric current flows from plate to plate, no matter how great the distance may be. The essential parts of a telegraph circuit are the battery, or generator of the current, the conducting-wire, the earth, and the instrument for recording signals. The circuit is open when there is a break in the wire, and closed when it is continuous and the current is flowing.

The time a current takes to flow from pole to pole in a circuit is imperceptible to the senses. A telegram can be sent from London to Glasgow or Paris, as quickly as from St. Martin's-le-Grand to Shepherd's Bush. During the recent repairs of the telegraph cable near Bombay, the steamers *Chiltern* and *Great Northern* were about half-a-mile apart, the former having hold of a shore-end cable, and so was in telegraphic communication with Bombay, the latter having hold of a sea-end, and so was in telegraphic communication with Aden. The *Chiltern* desired the *Great Northern* to splice on to the cable-end held by the latter, and pay out three-quarters of a mile of cable, and this was communicated by wire from the test room of the *Chiltern*, passing through all the coils of cable in her hold, and on to Bombay, whence it was sent on to Aden, and back from Aden to the *Great Northern!* Thus, as a *speedy* means of sending a message *half-a-mile*, it was forwarded by a route between three and four *thousand* miles long! The following morning, when the two vessels were within a quarter-of-a-mile of each other, communications passed between them constantly in the same way. Of all the miracles of modern science, truly this annihilation of distance is the most wonderful! The student will now understand how the electric current is generated, and conducted to its destination. He will, however, naturally desire to know how the electricity which flows through the circuit is made to record signals—in short, how intelligible communication is established between two distant places. All telegraph signals depend on the power of the telegraphist, who, to make them transmit, controls and modifies the current at will. Let us imagine a battery of several cells in the office of the editor of this journal in Great Queen-street, and suppose that we connect a wire to the positive pole, and carry it on posts to the Observatory at Kew, where the wire must be attached to a plate buried in the earth or connected with the gas-pipe. Join up the negative pole of the battery with a buried plate or gas-pipe, and the circuit will be complete. Cut the wire at any point between London and Kew, and the circuit will be broken—no electricity will flow through it. The current can be instantly re-established by bringing the cut ends together. Now, if we had some kind of apparatus that would make the currents visible, we could easily arrange a code of signals to represent the letters of the alphabet. There are now many ways of recording signals. Some of the instruments used are very complicated, and depend to a great extent upon ingenious mechanical contrivances. The simplest form of instrument is called a galvanometer, a modification of which is known as the receiving portion of the needle telegraph. To understand the principle of the galvanometer, the relations between electricity and magnetism must be studied. A very simple experiment will suffice to prove that electricity is influenced by magnetism, and *vice versâ*. A copper wire suspended horizonally over a movable magnetic needle will, when connected with the poles of a voltaic battery, cause the needle to place itself at right angles to the wire. If the current passes above the needle, and goes from south to north, the north pole of the magnet deflects towards the west. If the current passes below the needle, also from south to north, the north pole will deflect towards the east. If the current passes above the needle, but from north to south, the north pole is deflected towards the east. If the current goes from north to south below the needle, the deflection is towards the west. To enable the student to remember these movements, Ampère suggested the following rule:—If we imagine a man swimming in the wire, with the current, with his face turned towards the needle, the north pole of the needle will be deflected towards his left hand. According to Preece, "the motion of the needle is produced by the mutual action of currents and magnets. Electricity and magnetism are so intimately related to each other, that by many they are thought to be only different phases of the same agency. Thus the motion of a magnet always produces electricity; the transference of electricity always produces magnetism. The neighbourhood of a current is, in virtue of this fact, a *magnetic field*, a term introduced by Faraday to denote the entire space through which a magnet diffuses its influence—and a magnet or piece of soft iron placed there is influenced by the magnetism of that field." To increase the intensity of the current, the wire is wound several times round the magnetic needle, each coil being still parallel. Each successive coil produces a separate effect upon the needle, so that if there are a great number of coils of wire passed carefully before and behind the magnet, the deflecting force will be multiplied in proportion to the number of coils, and the most feeble current can be made to affect the needle. The fine wire used for this purpose is covered with cotton, silk, or some substance which is a non-conductor of electricity. If the coils of wire were not insulated, they would allow the current to pass from coil to coil, instead of following the continuous thread of wire. The wire is coiled round a frame, in the centre of which is suspended the magnetic needle; upon the same axis is fixed a steel indicator, which points to a scale, and this measures the strength of the current sent through the wire. The galvanometer is included in the circuit, and when contact is made, the indicator, which hangs in a vertical position when at rest, deflects in proportion to the intensity of the current. The single needle instrument, which is only a modification of the galvanometer, will be explained in a subsequent article.

REPLY TO QUERY.—[349]—Quotation, p. 502:—

"Little things
On little wings
Bear little souls to heaven,"

Are lines taken from a poem written by Father Faber.

TUBERCLE AND ITS CURE.—The consequences that will be witnessed as flowing from the studies conducted by Koch in connection with tuberculosis are impossible to foreshadow in any completeness, but we can, even at this early stage, perceive something of the widespread benefit that may ensue from them and their publication. Not least important of these must be considered the bearing which antiseptic treatment has been shown to have on the productive cause of the disease; the inference that is irresistible from a careful survey of the facts demonstrated by Koch's researches, indeed, is to the effect that we may very possibly find a future remedy for incipient, and even for pronounced, phthisis in the submission of the infected individual to active antiseptic measures. In this connection, an instructive and important communication has been made to the *Times* by Mr. R. R. Maddison, who states that, having proceeded to Madeira in the hope of simply prolonging life somewhat, but with absolutely no prospect of recovery from the consumptive condition, he resorted to the use of carbolic acid as a protection against mosquitoes. The vapour of the acid was necessarily inhaled by him, and to its beneficial effect on his lungs he ascribes an improvement in his state. He continued the use of the acid, and returned to England recovered, with the further consequence that he has remained well ever since. If, as seems quite justifiable, we may attribute this cure to the destructive effect of the germicide acid on the bacilli of tubercle, there opens up a very grateful prospect of possible relief in innumerable cases of lung disease; and at any rate it offers strong inducements to resort to treatment at once easy and possessing such promise of successful results.—*Medical Press.*

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XUM

THE COMET.

THIS week we give the comet's path (as computed by Dr. E. Lamp, of Kiel), from May 18 to the end of the month. As some correspondents who have equatorially-mounted telescopes are not altogether content with the mere mapping of the path, though the comet can now be readily seen in a good field-glass (we have seen it for the last fortnight in one of Browning's Panergetic Glasses) directed to its mapped place for the night, we append Dr. Lamp's positions in right ascension and North Polar Distance for the last half of May :—

Date.	R.A. h. m. s.	N.P.D. ° ′
May 13	0 41 31	15 56
,, 14	1 5 14	16 29
,, 15	1 27 30	17 11
,, 16	1 48 5	18 3
,, 17	2 6 52	19 2
,, 18	2 23 53	20 9
,, 19	2 39 14	21 22
,, 20	2 53 0	22 41
,, 21	3 5 21	24 5
,, 22	3 16 26	25 34
,, 23	3 26 24	27 6
,, 24	3 35 20	28 41
,, 25	3 43 27	30 20
,, 26	3 50 46	32 2
,, 27	3 57 24	33 47
,, 28	4 3 27	35 35
,, 29	4 8 57	37 25
,, 30	4 14 1	39 19
,, 31	4 18 39	41 15

The perihelion passage will take place on or about June 10, midnight, at which time the comet's distance from the sun will be only 61 thousandths of the earth's mean distance.

CHALCEDONY ENCLOSING LIQUIDS.—I have read with much interest the paper by the Rev. Mr. Wiggins on chalcedony enclosing liquids, in your issue of March 24. A specimen, such as he describes in the collection of Mr. Patrick Dudgeon, is referred to in Traill's "Treatise on Quartz and Opal," 1870, where a considerable quantity of water enclosed in the chalcedonic druse was lost by gradual exudation through the crystalline pores during the course of years, but which was partly restored. At Professor Heddle's suggestion, the nodule was immersed for a considerable time in water, under the exhausted receiver of a powerful air-pump; the air was thus exhausted from the interior of the nodule, and favoured the gradual admission of water upon the restoration of the atmospheric pressure.—MINERALOGIST.

THE FEVER TREE.—The *Eucalyptus globulus*, which is being introduced in California, has many qualities which recommend it to Eastern sylviculturists. It comes originally from Australia, where the tests of various soils and varying seasons have amply demonstrated the good qualities of the tree, and its rapid growth even under adverse surroundings. Trials of late years in Southern Europe have further verified these claims, and there is no reason why the tree will not flourish in every section of the United States. It is hardier than the chestnut, and, like the latter, it will grow in the rockiest soils. It is more independent of rain food than any tree known in this country, wet and dry seasons alike failing to affect its growth. Its wood is hard, somewhat of the nature of yellow pine, but firmer and stronger, and fit for use in ship timbers, while in Australia cabinet-makers, wheelwrights, and carpenters use it throughout their trades. The bark yields a febrifuge second only in efficiency to quinine, but superior in all medical qualities to cinchona. This quality alone must make the tree invaluable, and its culture here, to an appreciable extent, would settle for ever the vexed question of quinine duties. The rapidity of its growth is its most wonderful feature. It grows four times as fast as the American pine, and for all ordinary purposes is fit to cut in five or six years. For the Eastern and Middle States, where the lack of forest protection is not infrequently felt, no tree has been offered the culturist that can present so many primary points of vantage as the Australasian immigrant, and its general introduction and culture should only be a matter of but little time. It may be added, the tree yields fragrance, but produces no fruit or nut. Its beauty of form and luxuriant evergreen foliage are additional qualities that must recommend it in country or city.—*Frank Leslie's Illustrated.*

WEATHER REPORT, FOR WEEK ENDING SATURDAY, MAY 13.

STATION.	ABERDEEN.							LIVERPOOL.							VALENCIA.							LONDON.							LYONS.						
Day of Week.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.	Sun.	Mon.	Tues.	Wed.	Thurs.	Fri.	Sat.
Thermometer in Shade. Max.	48	48	49	54	61	56	57	55	59	55	53	60	66	55	55	57	57	55	58	57	62	64	65	60	59	68	70	65	71	69	70	54	64	72	81
Thermometer in Shade. Min.	41	38	36	37	43	40	41	43	43	41	45	51	46	44	43	44	51	52	52	45	42	43	42	40	46	47	50	38	51	54	45	41	?	52	46
Thermometer in Shade. Day's Range.	7	10	13	17	18	16	16	11	16	14	8	9	20	11	12	13	6	3	6	22	20	21	23	20	13	21	20	27	20	15	25	13	?	20	35
Barometer. Inches...	30	30	30	30	30	30	30	29	30	30	30	30	30	30	30	30	30	30	30	30	30	29	30	30	30	30	30	30	29	29	30	30	30	30	30
Barometer. 100ths...	·03	·20	·21	·24	·12	·03	·32	·97	·08	·33	·37	·25	·27	·37	·02	·22	·27	·39	·26	·31	·23	·97	·01	·43	·36	·34	·21	·35	·95	·96	·32	·45	·37	·16	·17
Wind. Direction	E	E N E	S	W	S W	N W	N W	S S E	E N E	S E	W N W	S W	W N W	N W	W N W	N N W	S S W	S S W	S W	E N E	E S E	S S W	N N W	N N W	S W	S W	N N W	—	N N E	N W	N N W	N	S	S
Wind. Force (0 to 12) ...	3	2	6	2	2	·8	5	1	1	2	2	13	3	1	3	1	6	4	3	3	3	1	2	1	2	1	2	0	1	3	5	7	3	1	1
Weather. Beaufort Scale*...	o	b c	o	b v	c	b	o	b c	m	o	b	b	b	b	b c	b c	o u	o m	o m	b	b c	b c	b m	m	o	—	c	f	b	o	b	b	b	b	b
Rain-fall. 100ths of inch..	0	3	0	10	0	39	0	0	0	0	17	0	4	0	4	0	0	8	1	1	0	0	0	0	0	0	0	0	0	32	12	0	0	0	0

* WEATHER.—*Beaufort Scale* is, b. blue sky; c. detached clouds; d. drizzling rain; f. fog; g. dark, gloomy; h. hail; l. lightning; m. misty (hazy); o. overcast; p. passing showers; q. squally; r. rain; s. snow; t. thunder; u. ugly, threatening; v. visibility, unusual transparency; w. dew.

Reviews.

"STUDIES IN MICROSCOPICAL SCIENCE." *

THESE studies ought to succeed, for they supply a real want—one which, in the case of microscopic study, can fortunately be supplied. The astronomer cannot, unhappily, send round specimen planets for study by those who possess telescopes of sufficient power. Nor can the chemist or geologist conveniently send out geological or chemical specimens for examination. But it is possible to send microscopic subjects, and this is what Mr. Cole proposes to do—viz., to issue weekly for the use of students, teachers, the medical profession, and others, microscopical preparations, together with finely-executed lithographs of the specimens thus sent out for observation. The first specimen is one of yellow fibro-cartilage from the pinna of a cow's ear, stained doubly in logwood and resin; it is shown in the lithograph magnified 33 times linear. The methods of staining are charmingly described, and the explanation of the structure, shape of cells, matrix, and other features of the specimen may be relied on as thoroughly accurate. In Number Two, the subject for illustration is a transverse section (through the internode of the stem of a first year's twig) of the copper beech, stained in carmine and iodine green, and magnified 25 diameters in the illustration. The bibliography of the subject is in each case very full. The work is thoroughly sound and good, and will delight practical microscopists.

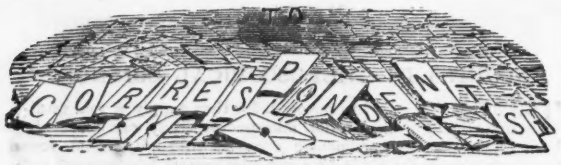
LAND AND WATER.

ALL the fossil-bearing rocks on the globe have been formed from the sediment brought down by rivers to the sea, and this sediment has been worn off from the hills and valleys and plains of ancient continents. In recent years it has been attempted to calculate the amounts of sediment worn off by various great rivers from the surface of the regions drained by them; and the results are very interesting and instructive. The Mississippi, for example, draining a country with scanty rainfall, and having its sources in the Alleghanies and the Rocky Mountains, where there are no glaciers, performs its work of denudation slowly. The Mississippi wears off from the whole immense area drained by it about one foot in 6,000 years; while the Po, on the other hand, having its sources in the glaciers of the Alps, works with great rapidity, and lowers the area drained by it at the rate of one foot in 729 years. The mean rate of denudation over the globe seems to be not far from one foot in 3,000 years. Now at this rate, and from the action of rivers alone, it would take only two million years to wear the whole existing continent of Europe, with all its huge mountain masses, down to the sea-level, while North America, in similar wise, would be washed away in less than three millions.

But while the raindrops, rushing in rivers to the sea, are thus with tireless industry working to obliterate existing continents, their efforts are counteracted, here and there, and with more or less success, by slow upward thrusts or pulsations from the earth's interior, which gradually raise the floors of continents. The general result of the struggle has been that, ever since the earliest geological periods, the surfaces of the great continents now existing have been subject to irregular oscillations; now partially or almost entirely disappearing beneath the sea, now recovering ground as archipelagos, or rising high and dry to great elevations, as in the case of Africa. The oscillations have not ordinarily exceeded from 6,000 to 10,000 feet in vertical extent. There is no reason for supposing that the general relative positions of the great continents and great oceans have altered at all since the beginning of the Laurentian period. Since life began on the earth there is no reason for supposing that the bottoms of the stupendous abysses which hold the waters of the Atlantic, the Pacific, and the Indian oceans have ever been raised up so as to become dry land. Once geologists thought otherwise, and land was turned into sea and sea into land by facile theorisers, as often as it was supposed to be necessary to account for the distribution of certain lizards or squirrels, or for changes in climate, such as have left marks behind in many parts of the earth. The greatest physical geologists now living, however—such as Mr. Croll and the brothers Geikie—are convinced that there has been no considerable change in the positions of the great oceans from the very beginning; and this view is ably sustained by Mr. Wallace—who is probably the highest living authority on the distribution of plants and animals—in his profound and fascinating treatise on "Island Life," lately published.—Mr. Fiske, in the *Atlantic Monthly*.

* "Studies in Microscopical Science." By Arthur C. Cole, F.R.M.S. (Baillière, Tindall, & Co.)

XUM



Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE*; all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

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**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE*, should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Letters which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

THE "CONEY" OF SCRIPTURE AND ITS ZOOLOGICAL POSITION.

[398]—"S. Lucas," in a letter addressed to the Editor, enters into a description of the resemblance which is alleged to exist between the rhinoceroses and the little hyrax, or "coney," of Scripture. Mr. Lucas also gives a comparison of the habits of the former animals as compared with the latter, and then asks how or why Cuvier included both in the same family. He adds, what of course no zoologist denies, that the teeth alone are by no means certain guides to the affinities of animals. Mr. Lucas, by consulting any recent manual of zoology, will readily satisfy himself that the hyrax is no longer placed near the rhinoceros-group; Professor Owen being the only authority, perhaps, who still places the hyrax-family definitely amongst the "odd-toed" hoofed quadrupeds like the rhinoceroses. No anatomist leans upon teeth alone as a guide to classification. Furthermore, hyrax exhibits even in its teeth important differences from rhinoceroses; and I may add that it was not so much the similarity in *number* of molars and premolars (as Mr. Lucas seems to think) that suggested rhinoceros affinities, but the shape and structure of these teeth. The hyrax has four incisor teeth, eight premolars, and six molars in each jaw, canines or eye-teeth being wanting. The upper incisors grow from permanent pulps, as in *Rodents*. The placentation of the hyrax is utterly unlike that of rhinoceroses, being deciduate and zonary, as in *Carnivora*. Again, the nails of hyrax are not hoof-like, but almost flat; and the upper lip is cleft, as in rodents. Hyrax is also remarkable for possessing more vertebræ in the back and loins than any other land mammal—the number being from twenty-nine to thirty-one. The modern view of the zoological position and affinities of the coneys is that which regards them as intermediate between the hoofed quadrupeds and the rodents. This is Brandt's view, and he adds that it is more ungulate (not necessarily rhinocerine) than rodent in its nature. Huxley says that the coneys lie between the hoofed quadrupeds and the rodents and insectivora. In any view, Mr. Lucas may rest assured that the position of hyrax is discussed and settled to-day from a consideration of its anatomy as a whole, and not from the shape or structure of its teeth alone.

Andrew Wilson.

CONSUMPTION.

[399]—Without infringing on the space of Knowledge, allow me to say in its columns, *apropos* of Professor Tyndall on "Consumption," that Koch does not show that *bacilli* are the *primes*, or first causes, or occasions of natural consumption; or that hereditary phthisis arises from them. He does not show that *bacilli* = scrofula; nor that in the course of these generations—their "culture"—they are more than mere carriers of an evil matter, influence, or incitation; but not the causes of it. Ordinary generation does not necessarily disown spontaneous generation, or *genesis*, as its beginning. *Bacilli* may be not the *principium* of the disease, but one of its consequences—perhaps a remote consequence,—capable, however, in rabbits, &c., of generating the disease after it has been their genesis. I think it is Wilson Fox who said that scratching rabbits with a sharp stick—*baculus*—will sometimes produce consumption in them, so liable are they to the peculiar rot of scrofula.

Trusting to your openness in Knowledge for the insertion of this note, which opposes such a sea of opinion, I am, yours obediently,

Garth Wilkinson, M.R.C.S.E.

[400]—If Koch's views, endorsed by Professor Tyndall, and published in the *Times*, should prove to be correct, a great social revolution will be brought about by them; for hitherto, with rare exceptions, pulmonary consumption, the chief of the tubercular diseases, has been looked upon as non-contagious, and its unfortunate victims have freely mingled with their family connections and with the people at large. But let these views be adopted by the medical profession, and generally received, and it will be imperatively necessary to keep them away, in great measure, from their fellow creatures, and to subject them to the same regimen as we use in the rest of the contagious diseases; and it is no small augmentation of their calamity that this regimen will require to be continued, not, as in other contagious diseases, for a few weeks, or months at most, but, in some instances, for many years.

Observing that you have transferred Professor Tyndall's letter, which appeared in the *Times*, to the pages of your magazine, I ask you to allow me to state a fact in connection with this subject which Professor Tyndall appears to have overlooked; and a fact which ought to make us cautious in accepting Koch's views on pulmonary consumption without requiring for them further consideration and research.

The fact I refer to is this,—that pus, brain, cheese, and other substances, when inoculated, will set up morbid processes in various organs, which cannot be distinguished from those which are set up by the inoculation of tubercle itself. If, therefore, pus, cheese, brain, &c., and tubercle itself produce the like results on inoculation, how can that which tubercle produces be specific? And, further, if *bacilli*, such as have been observed by Koch in tubercle, be also found in the rest of these products of inoculation, then, in all probability, they are effects, and not causes.

And finally, may not all the results obtained by Koch be due to a septic matter introduced along with his *bacilli*? If this be so, then the results obtained are *pyemic*, plus *bacilli*, now discovered, it appears, for the first time, and they are curious and interesting, but I fear they are nothing more; and, at least, they are far from being proved to be the *vera causa* of pulmonary consumption.

William Dale, M.D., Lond.

PROBABILITIES.

[401]—In your last article on "Probabilities," you discuss the fallacy contained in the assumption that A must win in the long run his original stake, provided that each stake is double the previous one, and I think the fallacy of such an assumption is sufficiently clear; but in the scheme I here submit, the fallacy, although just as real, is not just as clear to me, and I should be glad if you would make it so.

Make three columns, one for winning, two for losing, thus:—

W.	L.	L.
	1	
	3	
	2	
	1	
	2	2
	3	3
5	4	4
6	4	4
6		
3		

Write down at the top of the second column the number you wish to win in as many parts as you please, thus, you desire to win £7. Write 1, 3, 2, 1.

Commence by staking a small figure, say £1, and, say you win, continue to stake small until you lose. Make no entry of your first winnings, but enter your loss in both losing columns. Say you have lost a stake of £2. Your next stake is the sum of the top and bottom figures, £3. You lose; enter the loss as before. Your next stake is £4; enter as before. Your next stake is £5; you win. Enter this in the winning column, and cross out the top and bottom figures of the centre column. Your next stake is the sum of the top and bottom figures not crossed out in the centre column, or 3+3=6. You win this, and enter the 6 in the winning column. Proceed as before, your

next stake being £4, you lose, and enter in both losing columns. You next stake (4+2) 6, and win. Proceed as before, and stake 3; if you win, your scheme will have completed, as it were, one revolution, as *all the figures* in the centre column are crossed out; and your winnings are £20, against £13 lost, leaving you £7 to windward, as sailors say. If you lose, you must go on staking as before, until the figures in the centre column are crossed out, when you will be the winner of £7, *in every case!**

It is obvious that instead of 7, *any number* may be used, and may be divided into the greatest possible number of whole parts.

N.

INTELLIGENCE IN ANIMALS—JUMBO.

[402]—As another proof that we are not the monopolists of intelligence in the animal world, it is reported from America that just before Jumbo reached the truck in which he was to be confined, some planks, two or three feet high, had to be crossed. Before proceeding, however, he thumped them hard with his trunk, and then pressed them with his fore feet. Why should this reasoning in a brute be called instinct, when with us it would be called common-sense?

H'SETT.

THE LUMINOUS MIXTURE OF BLUE AND YELLOW.

[403]—Professor Rood's observation is not silly, as Colonel Ross states, but incorrect—a fundamental error made by Brewster.

Colour is a sensation bearing no resemblance to its physical cause. To mix colours is merely to excite simultaneously the same parts of the retina with the stimuli, which are known by experience to excite severally the sensations of colour it is desired to combine. Lambert's method obviously accomplishes this object. Other methods are: by rotation on the colour top; by simultaneous illumination of a white object, as with the double magic-lantern. Moreover, elaborate apparatuses have been devised for the luminous mixture of the pure rays of the spectrum. All these methods lead to the same results, which are accepted by all who have studied the subject, and are really as well established as any scientific facts.

To explain Colonel Ross' results would require a special investigation of the circumstances of each case. I would remind him, however, that, in order that blue and yellow may combine to form white, the blue must be exact in hue; the slightest leaning towards green will impart a green tinge to the result, which, in the flame method, may seem very conspicuous.

In regard to the last experiment—

(1.) The explanation cannot be true. The idea that the mere concentration of the beam can affect its refrangibility is quite contrary to what we know of physical optics.

(2.) If it were true, it would not prove the point. It would prove only that the complementary colour of a red formed from the extreme red rays is green, as indeed it is. If, however, Colonel Ross insists on the *grass* green, I think he is at issue with accepted facts on another point.

JOHN TENNANT.

DOES THE MIXTURE OF BLUE AND YELLOW MAKE GREEN LIGHT? FLUORESCENCE.

[404]—Letter 388, in KNOWLEDGE, for April 28, proves what difficulty is caused (in treating of colours) from the want of definite names and an invariable standard of reference. Lieut.-Col. Ross shows that by common usage, the term "blue" is applied to the chloride of copper flame; and the same might be said of the Bunsen flame. In the same way Newton gave that name to the colour which follows green in the spectrum, and called the deeper blue, which comes next, "indigo," and the still deeper and darker blue, which closes the series, "violet." Yet Newton's indigo, which is exactly the hue of artificial ultramarine (or French blue), is really a purer blue, differing from his other blue in containing less mixture of green; and Newton's violet, which is commonly supposed to have a tinge of red, and which some even term "purple," has been proved by most accurate observations (when viewed by itself) to be absolutely free from red, and from any appreciable quantity of green. When the flames above referred to are viewed through the prism, it is seen that they give out a large quantity of green light as well as of pure blue. When the blue part is neutralised by the addition of a yellow light, as in the experiments referred to in Lieut.-Col. Ross's first letter (p. 496), the greenness still remains, and appears in the resulting colour diluted with white.

The following most beautiful and instructive experiment, however, settles at once and for ever what is the colour which, when added to yellow, makes white; and it teaches far more than any experiments with coloured flames or pigments can teach. Lay two rectangular pieces of white paper so as to touch cornerwise over a dark cavity, the edges of the touching corners being in the same two cross lines, and in the same plane. Then view through a prism, held parallel to one of the cross lines, the spectra of the two white spaces. On one side we have the series of colours—red, orange, yellow—formed by combinations of the prismatic rays beginning at the red end of the spectrum; on the other side, the series formed by the rays wanting in the first—sea-green, sea-green-blue, and blue (the best and purest blue that ever the eye can behold). And in these two series the opposite colours must be perfect complementaries—the sea-green to the red, the sea-green-blue to the orange, the blue to the yellow, as may be seen by causing one piece of paper to slip forward so as to pass the corner of the other piece, when the overlapping colours all turn to white.

It is easy, therefore, to see that when we inquire whether mingled blue and yellow lights make white or green, we must distinguish between the different colours which are commonly called blue. As to the binary compound of green and blue, the complementary of red (which is exactly the hue of fresh verdigris powder), it is unfortunate that we have no good distinctive name for it. I have not met with a better term than "sea-green"; which term is certainly not derived from the appearance of yellow sand seen through green sea-water, but is the colour of a peculiar reflection of the sky from the sea—a tint hardly ever seen in nature elsewhere, except in a few minerals, and sometimes in the sky itself.

I beg pardon for so hastily suggesting a mistake in the experiment described by Lieut.-Col. Ross in his Fact 4. The green light he saw in the prism, when the focus of his lens was thrown upon it, was no mistake, but is a beautiful, and to me quite new example of the fluorescence so ably expounded by Professor Stokes in the *Phil. Trans.* for 1852. Glass is known to partially obstruct the invisible ultra-violet rays; but I do not recollect seeing it noticed before that this is attended with the production of a green luminosity in the glass. If the converging rays are made to fall on the flat end of a prism, so that the focus is formed in its middle, the luminosity may be seen extending from end to end of the prism, accompanied with two parallel reflections, more easy to understand than the reflections which complicate the experiment described on p. 496.

A very beautiful and bright sea-green-blue fluorescence is produced when a sunbeam is made to converge to a focus in a weak infusion of the bark of the ash or the horse-chestnut, or a weak solution of quinine; and in these cases the peculiar luminosity is strong enough to be seen in the fluid when the sun shines upon it, without the aid of a lens to intensify the incident light.

W. BENSON.

THE POTATO.

[405]—In No. 24, p. 520, "F.C.S.," replies to my letter in No. 20. He takes me for an English farmer. I have the pleasure of informing him I come from the North, and know both the practice and theory of my profession. I agree with "F.C.S." that we get many hints from our Continental neighbours, but at the same time deplore the fact that most members of "F.C.S.'s" body think they know farming, whereas very few of them can talk of farming matters without immediately saying something which every practical man knows to be nonsense, and when the practical man sees him talk nonsense on what he knows about, he very naturally concludes he also talks nonsense when he goes so deep into the subject that he cannot follow him.

With five experiments during 1879, 1880, 1881, cut and uncut flowers were as near as possible identical, viz., 8 tons 15 cwt. per imp. acre, on an average.

What I meant about the frosted potatoes was this: take say two samples of frozen potatoes from the same heap, cook one in the ordinary way, by boiling; roast the other in hot wood ashes; those from the latter will be good food, the boiled ones too sweet to be palatable, so that I say it is the frost coming out which makes the change, not going in.

I have tried many times most manures, pure, single, and compound on many crops, both on sand and clay. I see "F.C.S.'s" experience is second-hand, although not much the worse of that.

Among most agricultural experimenters, it is generally acknowledged that the presence of a large quantity of decomposing organic matter increases not only the crop, but also the percentage of disease. Now, "F.C.S.'s" experiment being on sand, there was likely to be little organic matter present, or if so, it was not a suitable station, and had it been on moss, the percentage of disease would also have been small; therefore I cannot see but that there is a contradiction in saying that where we have organic matter, and, of course, expect a large crop, that there we have most disease, and again in the unmanured plot with no organic matter and a small crop, that it is also the worst with disease. At least such I take to be his meaning.

* This is exclusive of what you gained before you began to lose.

XUM

All other things being equal, it may be taken as settled that the percentage of disease in the unmanured plots will be the smallest, or nearly so.

"F.C.S." gives no proof of his assertion that "good peat is equal to stable manure." I again say it is nonsense, and wonder anyone signing himself "F.C.S." should have uttered it.

This discussion was commenced in KNOWLEDGE, and unless the Editor objects, another paper is not required to finish it, but if "F.C.S." has been a regular reader of the *Field* in past years, he must have seen many of my contributions on the same subject, but with a different signature.

Would "E. W. P." advocate an excess of mineral manure so as to increase the starch in potatoes, and does he know if such an excess would also increase the starch in the cereals? FARMER.

AN ARTIFICIAL MOON.

[406]—The moon question having been somewhat prominent in your columns lately, a trial of the following experiment may prove interesting to some of your readers. I venture no opinion as to the connection in cause of the strongly-marked resemblance between the real moon and the artificial.

Take a soup plate, and slightly grease the surface with lard or oil; distribute irregularly in varying thicknesses about a table-spoonful of so-called granulated citrate of magnesia. Take a basin, pour in enough water to fill the soup plate; shake into the water about two-thirds the quantity of fine freshly-burnt plaster of Paris, which will sink at once; pour off nearly all the superfluous water; stir two or three times with a stick or spoon, so as to mix irregularly the paste; then pour it on the powder in the soup plate. The water in the plaster will cause an immediate disengagement of carbonic acid gas, which will rise in bubbles of various sizes through it in irregular patches; the plaster almost immediately setting, the shape of the outline of the bubbles and the walls of them become fixed, and, as a result, a most startling resemblance to the cratered surface of the moon is produced.

If a photograph of this be taken with a strong light, the resemblance becomes so perfect as to deceive almost all who are not professional astronomers. I believe that a little sugar, or syrup, or gum in the water would produce larger craters, but I have not tried this. A. STEWART HARRISON.

[As we have for several years used illustrations of the moon's surface formed by Mr. Harrison in the way described above, we can vouch for the accuracy of his statements.—ED.]

"THE STARS AND THE EARTH."

[407]—In your notice of the new edition of "The Stars and the Earth," you remark that the author is unknown. Many years since, two papers "On Good and Evil" appeared in *Macmillan's Magazine*. The editor stated that those papers were by the author of "The Stars and the Earth," Dr. Felix Eberty, of the University of Breslau. In the Catalogue of the British Museum, both the English and German edition of the book are under Dr. Eberty's name, and as the two papers in *Macmillan* display the same ability as is manifested in the book, the common authorship may be regarded as certain. J. W. F.

SIDEREAL TIME.

[408]—The following neat method of ascertaining (without an ephemeris) the approximate sidereal time at noon for any day of the year, may be useful to such of the readers of KNOWLEDGE as observe in the day time and have no rule of thumb way of finding sidereal time. It was communicated to me by Alfred Fryer, Esq., of Elm Hirst, Wilmslow, with whom, I believe, it was quite original.

From the month and day increased by twelve months when necessary substract three months twenty-two days, call each month of difference two hours, and each day four minutes. Thus for the day of issue of KNOWLEDGE, May 5—

5 months 5 days—3 months 22 days = 2h. 52m. sid. time.
Nautical Almanac shows ..... 2h. 53·1m.

OCEAN.

THE First Volume of KNOWLEDGE will be published early in June next, bound in red cloth, gilt lettered. Price 10s. 6d. Vol. I. will comprise the numbers from the commencement (Nov. 4, 1881) to No. 30 (May 26, 1882). As there is only a limited number of copies, the Publishers advise that orders should be sent in without delay, to prevent disappointment.

Binding Cases for Volume I. will also be supplied, price 1s. 6d. each. Complete copies bound (including case) for 2s. 6d. each.

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* KNOWLEDGE, *the increasing circulation of which compels us to go to press early in the week.*

HINTS TO CORRESPONDENTS.—*1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 4. Each letter should have a title, and in replying to a letter, reference should be made to its number, the page on which it appears, and its title.*

PHINEAS FOGG. Rat her too many queries, now that the query column has fallen. There will be 30 numbers in Vol. I., and as many in Vol. II., after which the volumes will each contain 26 numbers—two volumes to each year. This has been arranged in response to the request of several correspondents, that the first numbers of volumes should fall, hereafter, in first weeks of January and July. "Is Mr. Grant Allen a follower of Darwin?" Why, certainly,—being a man of science. The east wind is supposed to affect the health and spirits, because too dry; I cannot say I am satisfied with the reason, but the effect of the east wind is, unfortunately, not doubtful. — C. H. WINGFIELD. That my little joke about Mr. Proctor should have been misunderstood was fortunately of no moment; but the lesson is worth noting, as a misunderstood joke may be mischievous. I will venture to say not one American out of ten thousand would have misunderstood me.—H. C. Astronomers know nothing of the inclination of the asteroids' poles. Noise heard when shell is put close to the ear is generally supposed to be due to the warmth of the face, and consequent air currents. The explanation is doubtful. Other questions answered in elementary treatises.—H. BRAIN. There is every reason for believing that no former nations possessed anything resembling our modern knowledge of mathematics. The history of the progress of mathematics would fill many volumes of KNOWLEDGE.—J. F. G. Persons who have been born blind and afterwards received sight have been found to require several weeks' experience, touching and feeling objects near them, and so forth, before they acquire the power of distinguishing the relative sizes and distances of objects.—ZION. Do not know of any such collection.—W. H. M. There is not any difference between tossing a million coins at once, and tossing the same coin a million times running, so far as the antecedent probability of the relative number of heads and tails is concerned; but, of course, in tossing the same coin a million times running, the observed result of the earlier tossings modifies the expectation as to the final result. A portion of that which had been doubtful has become certain. You say that at the start, in tossing a coin 100 times, it is likely there will be 50 heads and 50 tails. On the contrary, this exact equality is unlikely. Again, you say if the first toss is head, it is 50 to 49 that the next toss will be tail. On the contrary, the next toss is as likely to be head as to be tail. The mathematical chance for the result of many trials is considered in this week's KNOWLEDGE.—V. WHEELER. Yes; but with similar requests for weekly instalments of ten or twelve different subjects, what are we to do?—STELLA. No one else appears to have seen the two large red spots which remained visible to you from 10.5 to 10.15 p.m. near Alpha and Beta, Ursæ Majoris, on the 24th inst. Are you sure there was no optical illusion? Were your eyes tired? or had you been looking at two dark green masses?—J. A. O. In treating of special subjects, writers can name the books which they recommend. Letters asking for the names of books on particular subjects should be sent to writers who treat of those subjects. Science knows nothing about the colour of Adam.—H. J. IVERSEN. (1) When a carriage is turning round a corner, the inside wheel tends to rise off the ground, because the body of the carriage has a centrifugal tendency outwards from the centre round which for a moment the carriage is turning. (2) In railroads, the outer rail at curves is raised so that this centrifugal tendency may be overcome by the tendency of the carriage to lean over towards the side on which the lines are lowest. (3) A turning carriage raises the inside wheel off the ground when the velocity is such that the moment of centrifugal tendency exceeds the moment of the carriage's weight around the lowest point of outer wheel. Thus, suppose M the mass of the carriage, h the height of the centre of gravity above the ground, v the velocity with which it is travelling (in feet per second), d the distance between the wheels, R the radius of the circle in which the centre of gravity of the carriage is moving. Then the moment of the weight round the outer rail or lowest point of outer wheel, is $\frac{Mgd}{2}$, the centrifugal tendency is $\frac{Mv^2}{R}$, and the moment of this tendency round the lowest

point of outer wheel is $\frac{Mv^2h}{R}$. Thus the limiting velocity is deduced from the equation—

$$\frac{Mgd}{2} = \frac{Mv^2h}{R}$$

$$\text{or} \quad v^2 = \frac{gdR}{2h}$$

This is, of course, only a rough treatment of the problem, obtained by regarding the carriage as if its whole mass were at the centre of gravity. It shows that for a given velocity the stability increases with the distance between the wheels and the largeness of the turning circle, and diminishes with the height of the centre of gravity above the ground. (In the above, $g = 32{\cdot}2$.)—E. D. G. Thanks; your method, and any common-sense method, is better than the ordinary rules for dividing decimals.—K. H. NISBETT. (1.) The moon does occult stars continually; but the Nautical Almanac only gives the occultations of certain catalogued stars. (2.) The stars in the field of view of a telescope are not so crowded that the new moon would seem like a dark circle on the starlit background, even if the new moon were black, which, however, is not the case.—E. D. ARCHIBALD. Many thanks for your letter and paper. There can be no doubt on the subject. I should be glad to hear from you at your convenience.—H. CLIFT. Many thanks; but we have not space.—S. HOPKINSON. I hope your bet was only imaginary, though, if made, it was certainly won. The question is, If the odds are the same on two independent events, and the betting is even that both happen, what are the odds on each separately? Let the chance of each be x, then the chance that both will happen is x^2; but this, on the supposition that it is even betting, is one-half, or $x^2 = \frac{1}{2}$, and $x = \frac{1}{\sqrt{2}} = {\cdot}707$ approximately. The chance of each event being 707-1,000ths, the odds on each event separately, are 707 to 293, or roughly 7 to 3.—W. WATSON. You object to severity, and probably to unfairness; and to illustrate your fairness and gentleness you characterise my answers to correspondents as a weekly dose of sulphuric acid, and contrast them unfavourably with those given in a contemporary paper. Now, I have taken the answers given under this heading in the last five numbers of KNOWLEDGE, and I find that roughly only about one answer in twenty is severe at all, ten or twelve neutral, and the rest useful replies to sensible queries. I have not here counted forty or fifty replies which, in my anxiety to meet the requirements of readers, have been obtained from experts, and paid for at the same rate as original articles. About one answer in forty since KNOWLEDGE first appeared has been *really* severe; that is, not meant for banter, such as any sensible person would either take good-humouredly, or answer in kind. I venture to say that every really severe reply has been more than merited. I have no fault to find with the answers in the journal you name; but I happen to know that the paradoxical and the idiotic, the sour-tempered and the malicious among the correspondents of that journal were long since weeded out by treatment far severer than any applied in KNOWLEDGE, greatly to the gain of that journal and its readers. When the same work has been done (more easily, I trust) here, our answers to correspondents will no longer need even that occasional drop of diluted sulphuric acid which your imagination has modified into pure and pervading vitriol.—SCHOOL-BOY. Thanks (you write a very bold hand for a school-boy). You are quite right; in our haste—for be it known that answers have to be very hurriedly written or not at all—in the "fourthly" example at foot of second column of p. 576, we *did*, as you say, shift the decimal point different ways, instead of the same way. The quotient, as you say, has only two integral digits. Good boy!—GRADATIM. You are right: Professor Huxley's remark that mercury expands more than the glass holding it, and that *therefore* the barometer is higher on a hot day, is unsound. The size of the tube in no way affects the height of the mercurial column. The diminution of the specific gravity of the mercury is, of course, the true cause of the greater height, *cæteris paribus*, on a hot day. His remark at p. 355, that the earth moving more rapidly as it approaches the sun, neutralises any augmentation of heat which may be due to increased nearness to the sun, is even more obviously incorrect.—COGITO. Yes: the word centrifugal (p. 565, col. 2, line 12 from the bottom) should obviously have been centripetal; as the "copy" was printed matter, the mistake should not have occurred.—H. E. KILBY. Hipparchus first suggested that method of determining the sun's distance. Theoretically sound, it fails utterly in practice, as explained in the first chapter of my treatise on the sun.—M. E. BENHAM. We must content ourselves with notices of men and women who have in some way advanced scientific researches.—C. MOON. Kindly send the trigonometrical method: if you will also send the "fly," I will try to get the information you require.—W. S. S. S. Place of Uranus can be determined from the zodiacal map in Part IV.—W. S. D. (Chronological.) Your Association cannot teach astronomers anything about eclipses, past or future; it is evident, on the contrary, that you have much to learn. Your views about eclipses, transits, &c., are such as beginners very naturally form.—INTRO-GEOGRAPHICUS. No, they do not correspond; if the celestial sphere were a plane, they might. The outlines of constellations have no fixed positions like those of continents and seas; but are carried along so as to include the stars shown in their proper constellations.—M. J. B. "Norma" must be the constellation you refer to; it was invented by Lacaille.—C. J. CASWELL. Thanks for kindly words. We note your question about Venus in daytime.—J. B. Question recently answered (in Answers to Correspondents).—E. W. WHITE. The subject is one of great interest; the influence certain; but that there is any magnetism in the matter more than doubtful.—J. MCGRIGOR ALLAN. Thanks, but it has been already done in our columns.—E. SEMMENT. We know of no treatise on the Flora of Singapore.—J. O. Do not know who is the publisher of J. R. Young's "Solutions of Cubic and Biquadratic Equations."—J. HAMER. The polar axis of a heavenly body is that axis on which it rotates, and the equator is that great circle of the body which is at right angles to the axis of rotation. Was not my note of admiration, then, justified by your remark that the moon, instead of rotating on its polar axis, rotates on an equatorial one? It is a contradiction in terms. If you had said, as I think you mean, that the moon rotates on an axis directed earthward, I should have understood you. You might then simply have said the moon's polar axis is directed towards the earth. The idea is erroneous, but not self-contradictory, like the other. The moon's axis is very nearly at right angles to the plane of her orbit round the earth, and she rotates in it once in each revolution round the earth.—BRUCE COOK (1). The angle ARB exceeds APB by the sum of the angles PAR, PBR (this follows obviously by joining PR, producing and applying Euclid I., 16). Similarly the angle ARB falls short of the angle AQB by the sum of the angles QAR, QBR. Hence, since angle PAR = QAR, and PBQ = QBR, we get, by adding, twice the angle ARB = angle APB + angle AQB or is constant.—Q. E. D. (2). For action of syphon we must refer you to elementary treatises. (3). If n is even, its square root will contain $\frac{n}{2}$ digits. If n is odd, its square root will contain $\frac{n+1}{2}$ digits.

Both cases are included in the formula.

$$\frac{1}{2}\left\{ n + \frac{1}{2} - (-1)^n \right\}$$

$$\text{or } \frac{1}{4}\left\{ 2n + 1 - (-1)^n \right\}$$

BIOLOGICAL.

"Yang fottin" (Aspull, near Wigan. Why don't correspondents at least write their pseudonyms plainly?). 1. There are no perceptible morphological differences between man's red blood-globules and those of the higher apes—indeed, no physiologist can venture to swear in a court of law which is the blood of a pig and which that of a man—so close are the resemblances. Differences there may and must be, but they are those dependent on minute structure, and not on any broad lines of bodily conformation. To all intents and purposes, the blood globules of man and higher apes are similar in size and structure. With the exception of the Camelidæ, all quadrupeds have round, nucleated blood globules. 2. So far as we know, the higher apes coincide nearly with man in their gestation. 3. Regarding the occurrence of puberty in the higher apes, we still require exact information. 4. The tail in the highest apes is relatively as rudimentary as in man. On *a priori* grounds, we should presume that in the embryo of a tailless ape the tail might persist longer than in the human embryo, but no exact observations have been yet made on the gestation of apes. 5. Man's teeth have no break or interval (a peculiarity shared in by the little lemur, *Tarsius*), whereas in the gorilla, for example, there is a break. The *number* is the same, and the arrangement similar in man and the gorilla; and even in a baboon, the same number and arrangement exist; but in both gorilla and baboon there are to be seen differences in the pattern and shape from those of human teeth. The dental formula of all the old world apes is, in fact, the same as that of man: 4 incisors, 2 canines, 2 premolars, and 3 molars in each jaw.

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Obituary.

WE regret to have to announce the death, at the early age of thirty-two, of a promising young science teacher and lecturer, Mr. Thomas Dunman, Lecturer on Physiology at the Birkbeck Institution, and Physical Science Lecturer at the Working Men's College. His early education was limited, but his reading was wide and his memory remarkably retentive. It was always in spite of his surroundings that he went on adding to his stock of knowledge. About seven years ago he took charge of the Physiology class at the Working Men's College in Great Ormond-street, where his class was one of the largest and most popular in the College. The practical results shown by the examinations at South Kensington attested the thoroughness of his teaching. Like success attended him in other courses of lectures in other branches of science. At the Birkbeck Institution, where he succeeded Dr. Aveling as Physiology Lecturer, his work was much appreciated.

In 1879 he published a very useful glossary of "Biological, Anatomical, and Physiological Terms," and four of his lectures had appeared, "The Mechanism of Sensation," "The Starlit Sky," "Prehistoric Man," and "Volcanoes and Coral Reefs." He contributed to Cassell's "Science for All," to Ward & Lock's "Universal Instructor," "Amateur Work," and several other publications.

He married early, and the effort to support his family by science teaching and lecturing may fairly be said to have cost him his life. During the past two years there were warnings that his energies were being too strongly taxed, but they were unheeded, and at the beginning of the present year brain troubles became markedly apparent, and he was obliged to give up work; but it was too late. He gradually grew worse, and died on the 9th inst., leaving a widow and two children, for whom he had been unable to make any provision.

Our Mathematical Column.

THE LAWS OF PROBABILITY.

By the Editor.

AS an illustration of the rules established in our last, take the following:—

There are in a bag three white balls and seven black balls; a ball is drawn at random and replaced, and this process is repeated five times; what is the probability that at least two white balls will be drawn?

Applying the rule, we must suppose $3+7$ expanded by the binomial theorem to the power 5, the complete expansion being thus written:—

$$3^5 + 5.3^4.7 + 10.3^3.7^2 + 10.3^2.7^3 + 5.3.7^4 + 7^5$$

then the fraction obtained by writing the first four terms of this expansion over the whole expansion represents the chance than at least 2 white balls will be drawn. The whole expansion is, of course, equal to 10^5 or 100,000; and the sum of the first four terms is easily found to be 47,178, so that the required probability is $\frac{47178}{100000}$, or nearly one-half.

It need hardly be remarked, however, that the practical application of this rule is not always quite so easy as in the above instance. Tables have been constructed for the determination of approximate values when $n+m$ is large and direct calculation out of the question.

Of course, the chance that at least two black balls will be drawn is given by taking the *last* four terms of the expansion for numerator. In this case the calculation is even easier than in the former, though it would be less easy if the student proceeded directly to calculate the value of the four terms, and then to add them together. There is no occasion for this, however, for he knows that the total expansion of 10 to the power 5 is 100,000, and he has only to deduct from this the sum of the first two terms—that is, 3,078, leaving 96922. The required probability is therefore $\frac{96922}{100000}$, or more than twice as great as that of drawing at least two white balls.

It is easily seen that by precisely such reasoning as we have used to establish the law discussed above, we can obtain the following law:—

If at each trial there are $p+q+r$ possible results, all equally likely to occur, of which p are of one kind, q of a second, and r of a third, then the chance that in $(n+m+l)$ trials n are of the first kind, m of the second, and l of the third, is represented by the fraction—

$$\frac{\lfloor n+m+l}{\lfloor n \ \lfloor m \ \lfloor l} \cdot \frac{p^n \ q^m \ r^l}{(p+q+r)^{n+m+l}}$$

Here, too, as in the former case, the expression for the probability is divisible into two parts: a denominator, the expansion of $(p+q+r)$ to the power $(n+m+l)$; and a numerator, the term of this expansion involving $p^n \ q^m \ r^l$. And if we require the probability that at least n of the results will be of the first kind, and at least m of the second, we must for a numerator add together all those terms in the expansion of $(p+q+r)$ to the power $(n+m+l)$ which involve p^n, p^{n+1}, p^{n+2}, &c., and *also* q^m, q^{m+1} q^{m+2}, &c., that is, all terms in which the power of p is not lower than n, and the power of q not lower than m; so if results of the first and third, or of the second and third kind are in question. Of course, if we only require to know what is the probability that n, at least, of the results will be of the first kind, the problem belongs to the former case.

The extension of these considerations to cases where there are four possible classes of result, or five, or more, will be a simple matter to the algebraist. The following example will be more interesting to the general reader than a mere statement of the law; but it will be well to notice that the formula for all such cases bears precisely the same relation to that last given that this formula bears to the former.*

The letters forming the word "Mississippi" are marked on eleven tablets, all similary shaped, and placed in a bag. A letter is drawn from the bag at random and replaced; and this is repeated twenty-three times; what is the probability that these twenty-three drawings will give 3 m's, 8 i's, 7 s's, and 5 p's.

The bag contains 1 m, 4 i's, 4 s's, and 2 p's, or eleven letters all, and the required probability is—

$$\frac{\lfloor 23}{\lfloor 3 \ \lfloor 8 \ \lfloor 7 \ \lfloor 5} \cdot \frac{1^3.4^8.4^7.2^5}{11^{23}}$$

which may be written—

$$\frac{\lfloor 23}{\lfloor 3 \ \lfloor 8 \ \lfloor 7 \ \lfloor 5} \cdot \frac{2^{35}}{11^{23}}$$

and is readily calculable by logarithms.

The value of the probability in this and all similar cases is not changed when the number of possible results of each kind are multiplied in the same proportion. Thus, if the bag contained 20 m's, 80 i's, 80 s's, and 40 p's, we should obtain the same value for the probability above required, as in the case actually described.

When the number of possible results of each kind is very great indeed compared with the number of trials, we get appreciably the same probability whether after each trial matters are restored to their condition before the trial or not. Thus if a bag contain a million red balls, a million white, a million black, and a million green balls, we should get the same probability for the result of twelve drawings (say) whether after each drawing the drawn ball were replaced or not. The difference, at least, is not appreciable. Hence we get the same probability as respects a single trial in which twelve balls are drawn at once, as for twelve several drawings (followed by replacement).

Next week we propose to give the solutions of several problems which have been standing over for some time. Our papers on "Probabilities" will probably be concluded in the two first numbers of Vol. II.

Our Chess Column.

THE INTERNATIONAL VIENNA TOURNAMENT.

[*By Telegram.*] Café Reichsrathpark, Vienna, Tuesday night.

The following is the score of the English players:—

Mason	5½	Blackburn.....	4
Mackenzie	5	Steinitz.....	2½
Zukertort.....	4½	Bird	½

We have grave apprehensions that Steinitz's health must have broken down. On Friday, when playing with Captain Mackenzie, he overlooked that he could win a piece on his twenty-second move—the consequence being that the game was drawn. On Saturday he fared still worse; he lost to Zukertort. His score then stood at 2½. On Monday he had to play Kruby, and on Tuesday Ware. As

* At p. 349, No. 16, will be found a convenient formula for the expansion of a multinomial to a positive integral power.

is highly improbable that he should have lost to both of these players, we conclude that he did not play at all, or our telegram became distorted in transmission. The best score has undoubtedly been made by Captain Mackenzie, who, although ½ game less than Mason, nevertheless has relatively done best, he having beaten Winawer in the first round, then drawn with Zukertort and Steinitz, and beaten Paulsen. Mason has not encountered any of the best men except Paulsen. It is a curious coincidence in the above score that the youngest man heads the list, while the oldest player stands lowest.

Play began on Wednesday, the 10th inst. Specially noteworthy in the first week is, firstly, the encounter between the two great rival masters, Steinitz and Zukertort. The latter had the satisfaction of conquering his mighty opponent. Secondly, the meeting of Steinitz and Blackburne—the former won. Thirdly, the encounter of Captain Mackenzie, the representative of America, with Steinitz and Zukertort, with both of whom he drew.

It is a matter of astonishment and regret that we do not have daily information of the Tournament in the press. There can be no doubt that thousands of chess players in this country, who follow with eager interest this gigantic tournament, and in particular the fortunes of the English contingent, would be glad to have some information through the daily press. No less than five English players have gone to Vienna. Surely they deserve at least as much notice as a couple of tramps would get who run, hop, or jump around Lillie Bridge grounds.

We have much pleasure in giving a smart game played by Captain Mackenzie against Herr Winawer, of Berlin:—

GAME 643.

(Played in the first round of the Vienna International Tourney, May 10, 1882.)

Ruy Lopez.

WHITE. (Captain Mackenzie.)	BLACK. (Herr Winawer.)
1. P to K4	1. P to K4
2. Kt to KB3	2. Kt to QB3
3. B to Kt5	3. Kt to B3 (*a*)
4. P to Q4	4. P takes P
5. Castles	5. B to K2 (*b*)
6. P to K5	6. Kt to K5
7. R to Ksq	7. Kt to B4 (*c*)
8. Kt takes P	8. Kt takes Kt
9. Q takes Kt	9. Castles
10. Kt to B3	10. Kt to K3 (*d*)
11. Q to K4	11. P to QB3 (*e*)
12. B to Q3	12. P to KKt3
13. B to R6	13. R to Ksq
14. QR to Qsq (*f*)	14. P to KB4
15. Q to B3	15. P to Q4 (*g*)
16. P takes P *en pass.*	16. B takes P
17. B to B4 (*h*)	17. B takes P(ch)
18. K to Bsq	18. Q to R4
19. B to Kt3 (*i*)	19. Q takes B
20. P to Kt3 (*j*)	20. Q to Bsq (*k*)
21. K to Kt2	21. B takes P
22. Q takes B	22. K to Rsq
23. R to KRsq	23. R to K2
24. R to Q6	24. P to B5
25. Q to Q3	25. P to B6(ch)
26. K to Bsq	26. Q to B4
27. R to Q8(ch)	27. K to Kt2
28. Q to Q6 (*l*)	28. Q to Kt4
29. R to Ktsq	29. Q to QB4 (*m*)
30. R to Kt8(ch) (*n*)	30. K takes R
31. Q takes Q	Resigns

NOTES.

(*a*) We do not pretend to decide analytically the merits of this defence, we merely say we prefer P to R3. As a remarkable fact, we have seen a great many games prematurely break down—this defence having been adopted; noteworthy amongst them being one at Berlin, where Winawer defeated Dr. Schmid in twelve moves.

(*b*) This is better than Kt takes P.

(*c*) Had Black played the defence of 3. P to R3, the White Bishop in the usual course would have retreated to R4, and, therefore, after Black's seventh move Kt to B4, that Bishop would be attacked and compelled to move, thus giving Black time. Upon this fact we base our opinion, that 3. P to R3 is a good defence.

(*d*) The Knight is also disadvantageously placed on K3. We think Black might have played P to Q3 instead of Kt to K3; it would have better developed his game.

(*e*) The very thing White wanted; he now brings his Bishop into activity at the cost of Black's time.

(*f*) Now White's superiority is established, R to Qsq is very good; it further weakens Black's Queen's file in combination with the Pawn on K5. Black's defence, therefore, turned out badly.

(*g*) A desperate effort to force his cramped position, but risky, in view of the position of White's Rooks.

(*h*) Overlooking the palpable rejoinder of Black; a waiting move, such as P to KR3, would have done good service to White.

(*i*) In case White should have made an effort to retrieve his lost fortunes by B takes Kt, B takes B, and then retire his Bishop, Black would have a winning check with his B on B5; but through the move in the text White also loses two Pawns, which defence turned out more fortunate for White than could be expected.

(*j*) White relied upon this move to regain the piece.

(*k*) Surely Black had a straight road to victory by Q to R6(ch), and on Queen interposing exchanging. K to K2 would have been too dangerous for White to venture on, after exchanging Queens, and Bishop takes Pawn, Black would be two Pawns ahead.

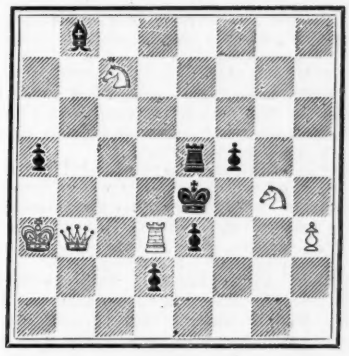
(*l*) White is playing well, and makes the utmost of his attack; while Black is evidently playing carelessly.

(*m*) This loses the Queen; he might have played Q to R5. White could not then have played B takes Kt, on account of Black's reply of B takes B, threatening B to B5(ch).

(*n*) Highly ingenious. Black has no choice. If K to B3, Kt to K4 wins, or if K to R4, R to Rsq(ch), followed by Kt to K4(ch).

PROBLEM No. 42, by J. A. Miles.

BLACK.



WHITE.

White to play and mate in three moves.

Mr. J. A. Miles intends publishing a collection of his "Problems, Poems, &c."—the problem above being one of the number. A selection of such excellent compositions is sure to be favourably received by the Chess public.

SOLUTION.

PROBLEM No. 38, by J. A. Miles, p. 562.

1. P to Kt4	1. K to Q4
2. Kt to Kt2	2. K takes P
3. Kt to B4 mate	
	or if 2. K to K5
3. B to B3 mate	
	or if 2. K to B5
3. Kt to K3 mate	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

Muzio 3.—Your best move is 8. $\frac{\text{B to R3, if then 8. Kt takes P}}{\text{Q to Kt3}}$, or if 8. $\frac{\text{P to Q3}}{\text{Kt to K5}}$ 9. $\frac{\text{Kt to K5}}{\text{Kt to K5}}$ 10. $\frac{\text{P takes P}}{\text{Kt takes P}}$ 11. $\frac{\text{R to Ksq(ch)}}{\text{Kt to K2}}$ 12. $\frac{\text{Kt to Kt5}}{\text{Castles (best)}}$ 13. $\frac{\text{Kt takes BP}}{\text{}}$, &c., (2.) 12. B to Q5 is probably best; thanks for problem; solutions of No. 38 and 39 correct.

Correct solution of Problem No. 39 received from Senex Solitarius, G. W., W. C. Thomas, and Gos.

Edward P. Westlake.—1. Q to B5(ch), K takes Q; 2. QKt takes QP, and 3. P to Kt4 (mate); if 1. K to K2, 2. Q takes P(ch) and mate next move.

Alfred B. Palmer.—Games received; will find you an opponent as soon as possible. Solutions of Nos. 40 and 41 correct.

Bright (Genoa).—Solution of Problem No. 38 correct.

Leonard P. Rees.—Thanks for analysis, which will receive our best attention; our services are always at your disposal.

H. A. N.—Problem received with thanks. Solutions of Nos. 40 and 41 correct.

Henry Planck.—Solutions of Nos. 38 and 39 correct.

G. W.—Solutions 40 and 41 correct.

Brenton.—Solution of No. 39 correct.

Our Whist Column.

By "Five of Clubs."

Play Third Hand (Plain Suits).

MANY players seem to think the only rule necessary for third hand is to play the highest card, unless the suit is headed in the hand by a sequence, when, of course, they do not carry the rule to so absurd a length as to play the highest of the sequence. If to this rule they make one exception, in finessing the Queen with Ace, Queen, they suppose they know all that need be known about third hand play.

In reality, however, play third hand requires considerable judgment, and a thorough knowledge of the leads and of play second hand. In two-thirds, perhaps, of the cases that arise it may suffice to know that third hand should play his highest, unless, of course, he cannot play higher than his partner, or only a card which is the next in sequence above his partner's, when he plays his lowest. If his suit is headed by a sequence, he plays the lowest of the sequence (with the same exception that if his partner's card is higher, or belongs to the same sequence, he plays his lowest, unless he has such strength in the suit that he may with advantage take his partner's trick). But in other cases, the player third hand has to consider the lead, the play second hand, and the score.

Suppose, for instance, your partner has led Queen, and that the lead is original, or at any rate that there is no reason to suppose it forced. Thus, the lead is presumably from Queen, Knave, ten, with probably one small card at least. Then, if you have the Ace and one or more others, third in hand, how should you play if second hand does not cover? You know in this case that second has not the King, and the first idea would be that, since fourth player must hold the King, you should play the Ace. But in general this would be wrong. The state of the score might render it advisable to take the trick lest second round should be ruffed. But usually it is best to let the trick go the fourth player. By putting on Ace, you sacrifice Ace and Queen for one trick, and leave the best card in the adversaries' hands. Apart from ruffing—which, be it remembered, always means a trump drawn from the adversary—the King will make; that is, the adversaries will have one trick in the suit in any case, and it is far better for you that that one trick should be in the first than in the second round. Consider the effect (1) of putting on the Ace and (2) of passing the Queen, apart from ruffing. In case (1) Ace makes first round, King makes second round, and another suit is immediately led—as likely as not the suit is not led again; in case 2, King makes first round, Ace takes the second trick, the suit is probably led a third time by holder of Ace, and in that case two more tricks are made in it, or trumps are forced from the enemy.

Again, suppose ten is led and passed by second player, you know (see our account of the leads in Parts I. and II., or our synopsis of them in No. 14, p. 310), that the lead is from King, Queen, Knave, ten, or from King, Knave, ten, with or without small ones. If, third in hand, you hold the Ace, when ten is led, you put it on, leaving your partner to finesse (if he holds King, Knave) on the return of the suit. If you hold Ace, Knave, you know that the ten is led as a strengthening card; you pass it, and even if the finesse fails, as is probable (for if King, Queen were both with second player the Queen would be put on unless he were long in the suit), you remain with the tenace. If when ten is led you have nothing above it but the Queen, you pass it: for whether it has been led (as is most probable), from King, Knave, ten, &c., or is a strengthening card, the play of the Queen would be bad: in the former case, obviously; in the latter because by playing the Queen you give up at once the command of the suit.

These illustrations suffice to show that the general rule, Third in hand play your highest, is as insufficient as we have already seen that the general rule is for second play, Second in hand play your lowest. We shall, therefore, proceed to consider the play third in hand,—first on general principles, and then in detail, as we have already considered the play of the first and second hands.

G. D. Brown remarks on the increased interest of Whist when honours are not counted, and gives the following short way of describing the double method of scoring.

"In addition to the usual score of the games another is kept of the balance of tricks throughout the rubber, which balance is paid for at the end, each trick counting one point in addition to (or sometimes in subtraction from) the points of the rubber."

In play the effect "is that instead of a hand being thrown up when a game is seen to be lost, the hand is played out in order that all the tricks possible may be made."

To the true lover of the game, who desires to see the element of chance as far as possible eliminated, counting honours, especially full honours in short whist, is always objectionable. But for one true lover of whist there are ten who love the excitement better which the element of chance introduces. For my own part, if it were not for the extra time it would involve, I should like to have all the honours of all the suits set together in a group of sixteen, and this set shuffled; the remaining cards being also shuffled; then the two sets being put together, and the cut made without further shuffling, every player would have four honours from among the four suits, and nine plain cards, in every hand. There would then be very seldom those cases of overwhelmingly good or bad cards between two partners which render good play either unnecessary or useless. This, of course, is heresy in the eyes of those who care more for the stakes than for the game, and more for the excitement of chance than for the *gaudia certaminis*.

Five of Clubs.

PROBLEM IV. (p. 506.)

The Great Vienna Coup.

The key to this problem, interesting as having occurred in actual play—though we venture to demur to the statement that the holder of the winning hands said he should make every trick *as soon as he had seen the hands*—consists in forcing the opposite hands to discard from one or other of the suits which seem to be perfectly guarded. A takes out three rounds in trumps, then leads his small trump. If now second player discards either a Spade or a Diamond, there is no difficulty, as he thereby unguards the suit from which he discards. If second player discards a Heart at the fourth round, he equally unguards that suit; but, owing to the position of the other two suits, it would not do for A now to lead a Heart. He must first lead the Ace of Spades, then a Heart, discarding Queen of Spades at the first opportunity. The rest is obvious. All the solutions we have received have been correct—indeed, one of the features of this double-dummy puzzle is that it is not easy to suppose one has solved it when one really has not.

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